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# THE PHILOSOPHICAL
# HISTORY
## AND
# MEMOIRS
### OF THE
Royal Academy of Sciences at *Paris* :

#### OR,

An ABRIDGMENT of all the PAPERS relating to *Natural Philofophy*, which have been publifh'd by the *Members* of that *Illuftrious Society*.

With many Curious OBSERVATIONS relating to the Natural Hiftory and Anatomy of Animals, *&c*.

Illuftrated with COPPER-PLATES.

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The Whole Tranflated and Abridged,
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#### AND

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## VOL. IV.

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### LONDON:

# AN
# ABRIDGMENT
### OF THE
## Philofophical Memoirs
#### OF THE
### ROYAL ACADEMY of SCIENCES at *Paris* for the Year 1710.

X. *Obfervations on the variation of the needle, with regard to Dr.* Halley's *chart : together with fome geographical remarks made from fea-journals, by* M. Delifle *; tranflated by Mr.* Chambers.

FAther *Gouye* having communicated to me 8 feveral journals of pilots, who fteered fhips from *France* to *Newfoundland*, and the *American* iflands, I extracted from them what appeared of moft advantage to navigation and geography ; and having the ufe likewife of two other journals, one of them kept by M. *Hebert*, his majefty's envoy to the *Indies*, and the other by M. *Bigot de la Cante*, fecond lieutenant on board the king's fhip the *Sphere*, to the coafts of *Guinea* and river *de la Plata*, I have thought proper to join all the obfervations together made in thofe feveral voyages, becaufe all made nearly in the fame time, *viz.* in the years 1706, 1707, 1708, and 1709.

* July 16, 1710.

The obfervations of the variation of the needle are of late become fo effential to navigation, that the pilots take all occafions of making them ; in effect, they cannot know the rumb in which they fail, without firft obferving the variation ; and when the weather will not allow them to obferve it, they are obliged to compute it from the obfervations of other navigators about the fame places. They even begin to make ufe of it for correcting their reckoning, and finding, in fome meafure, the longitude, when, either by themfelves, or others, they know the variation which fhould be found in fuch and fuch places. Thus M. *Daumas*, firft pilot of the *St. Louis*, difcovered hereby, that he was paffing the line more weftward than his reckoning gave him, and in the latitude of the ifland *Cape Verd*, he perceived by the variation, that he was 30 leagues more weftward than by his reckoning. He adds, that upon approaching the ifland of *Bourbon*, in the *Indian* fea, finding the variation to be 21°—— north-weft, he learned hereby, that he was to the weftward of that line, when his reckoning could not tell him fo much.

It would be needlefs here to relate the computed variations, our knowledge on this head is much too fcanty to make a juft computation ; I fhall here therefore confine myfelf to fuch as have actually been obferved, among which I fhall always prefer thofe obferved by the amplitudes to thofe by the azimuth ; the former appearing furer, and lefs fubject to error.

The longitudes I make ufe of are reckoned from the pike of *Teneriff*, according to the charts of *Pieter Goos*, which are thofe commonly ufed by our late navigators, for fetting off their courfe.

At

At 120 leagues from the coasts of *France*, and 44°———¼ latitude, one of the pilots, in the year 1709, found the variation 8°——— northwest by the sun's amplitude; and M. *Daumas*, in the same place a little before, found it the same. Dr. *Halley*'s chart makes it 6°———½.

At 45°———7'——— latitude, and 11°———31'——— longitude, he found the variation 6°———40——— where Dr. *Halley* makes it 6°———½.

At 45°———20'———latitude, and 358°——— 15'———longitude, he found the variation 11°——, where Dr. *Halley* only makes it 9.

Another pilot, setting out from *Rochel* for the *Cape Verd* islands in 1708, found at 35°—35'— latitude, and on the meridian of *Teneriff*, the variation to be 4°———35——— by the western amplitude: Dr. *Halley* makes it 4°———

At 27°———58'———latitude, and 353°——— 40'———longitude, he found the variation 4°— 32'— where Dr. *Halley* makes it 2°———10'.———

At 36°———lat. and 325°———46'———long. he found the variation 5°———8'———— where Dr. *Halley* makes it 3°———½.

Another pilot at 46°———50'——— latitude, and 230 leagues distance from *Rochel*, found the variation in 1709, to be 7°—50'— where Dr. *Halley* makes it 7°—30'———

Soon after at 260 leagues from *Rochel*, and 47°—latitude, he found the variation 8°——— where Dr. *Halley* likewise makes it 8.

At 33°———45'———lat. and 5°——— long. he found the variation 6°——— where Dr. *Halley* only makes it 3°———¾.

The first pilot of the *Marianne* at 43°—45'— lat. and 340°———46'———long. found the variation in 1709, to be 13°——— where Dr. *Halley* only makes it 8.

All

All thefe obfervations were made on this fide the line of no variation, fuppofing them exact, the variations muft have increafed on this fide the line, fince the time of Dr. *Halley*'s chart ; but lefs in one place than another.

M. *Bigot de la Cante*, in a voyage to the river *de la Plata* in 1707, found at 44°——45′——lat. and 52 leagues diftance from *Cape Finifterre*, the variation to be 7°——20′——north-weft, where Dr. *Halley* only makes it 6°——15′ ; and the fame variation he found in the 4 enfuing days, during which he made 60 leagues fouth-weft-wards. In Dr. *Halley*'s chart, it is 6°—— all along this line.

At 7°——15′——lat. and 1°——50′—— long. he found the variation 2°——30′—— where Dr. *Halley* only makes it 50′——

In the road of *Judda*, on the *Guinea* coaft, he found the variation in 1708, 8°——20′. M. *Des Marchais*, in 1705, found it 8°——. In the fame place, Dr. *Halley* only makes it 5°——

In the eaftern part of the ifland of *San-Thomas* under the line, he found it 11°—30′—— where Dr. *Halley* only makes it 5°—30——

From hence M. *Bigot* failed fouth-eafterly to 4°——fouth lat. not far from the coafts of *Congo*, from whence he always bore to the fouth-weft, and weft-fouth-weft, as far as the mouth of the river *de la Plata*, where he arrived in *April*, 1708. This traverfe of 14 leagues is the fitteft that could be thought of for examining the variations affigned by Dr. *Halley* in this fea ; and the more fo, as it cuts all Dr. *Halley*'s lines al-moft perpendicularly.

Along this courfe, the variation, which was north-weft, grew lefs and lefs every day, till having made 560 leagues, it entirely difappeared, and

and henceforwards the variations were north-weft. This line of no variation Dr. *Halley* makes 120 leagues more eafterly, than M. *Bigot* found it, and makes a variation of 1°——30′——north-eaft, where M. *Bigot* found none; fo that we are not to wonder, if the variation, found by M. *Bigot* before his arrival at this line, were greater than that affigned by Dr. *Halley*, by 1 or 2°—— and fometimes more.

In *April*, 1709, M. *Bigot* left the river *de la Plata*, and purfued much the fame courfe as he had kept before, for the fpace of about 800 leagues; but the obfervations he made in returning do not agree with thofe made in going, tho' by eftimation he thought himfelf in the fame places, where he had obferved before. The place where in going he found the variation 20′—— north-eaft, appeared by his eftimation 9°—more eafterly, than that where he found the variation 26′ in his return, and the other places in proportion to their diftance from the mouth of the river *de la Plata*, which probably arifes from the waters of that huge river, which communicating its current to the fea, by a mouth 30 leagues broad directed to the eaft, may be fuppofed to retard the motion of the fhip in going, and accelerate it in its returning.

From hence M. *Bigot* failed to *Martinico*, and in his paffage thence to *France* in 1709, at 28°—30′—lat. and 316°—30′—long. found the variation 1°——30′—north-weft, where Dr. *Halley* makes it 1°—north-eaft; fo that the variation has here changed from north-eaft to north-weft, and the line of no variation, which was eaftwards of this place, has travelled to the weftward thereof fince Dr. *Halley*'s chart, if we may credit thefe obfervations.

At

At 32°——15′—lat. and 321°—45′—— long. he found the variation 4°———10′, where Dr. *Halley* makes it 2°——— lefs.

At 36°——50 lat. and 329° long. he found the variation 7°—10′, where Dr. *Halley* makes it 4°——30′.

At 45°—8′ lat. and 305°—30′ long. he found the variation 10°—10′, which is 2°——more than in Dr. *Halley*'s chart.

From all thefe obfervations, fuppofing them juft, it appears, firft, that in the parallel of 22°—fouth latitude, the line of no variation has fhifted 120 leagues weftwards, from the year 1700 to 1708.

Secondly, That the variation has increafed on this fide that line, while it has diminifhed on the other|; tho' in three or four places M. *Bigot* found it the fame as is expreffed in the chart ; and in fome others found it even greater, unlefs we rather choofe to caft the irregularity on the obfervations, which I believe the fafeft way.

The fhip *St. Louis* went with the *Golden-fleece*, and the *Maurepas* went in 1706 for the *South-fea* ; the obfervations made on board this laft have already been delivered by M. *Caffini* ; but the firft quitting the other two on the coafts of *Chili*, carried M. *Hebert* on to the *Indies* ; in which voyage, befides M. *Daumas* the pilot's journal, I have another kept by M. *Brunet*, an officer in the fhip, who made feveral curious remarks, over-looked by M. *Daumas*.

This pilot putting out from *Port-Louis*, in *July* 1706, at 25 leagues north-north-eaft of the ifland of *Porto-Santo*, near *Madera*, found the variation 5°—north-weft, where Dr. *Halley*'s chart only makes it 4.

Clofe

Clofe by *Madeira* on the fouth-weft, he found it 4°——½, where Dr. *Halley* only makes it 3°——½.

Between the ifland of *Madeira*, and that of *Faro*, he found it 4, where Dr. *Halley* makes it 3.

At 50 leagues fouth-fouth-weft of the ifland of *Faro*, he found the variation 3, where Dr. *Halley* makes it 2.

At 18°——15' lat. and 357° long. he found the variation 2°——½, between which place and the bank of *Bifagos*, on the coaft of *Guinea*, he obferved the variation 4 times, and found it always 2°——½ ; Dr. *Halley* makes it in thefe places about 1°.

At 358°—long. and 6°——lat. he found the variation 2°——, as well as at 3°——15' lat. and 10' long. in both which places, Dr. *Halley* makes the variation about ½ a degree.

From this place, bearing to the fouth-eaft, as far as the equinoctial, which he cut at 7°— long. the variation in about 50 leagues courfe changed from 2 to 3°—— at 50 leagues further from 3 to 4°—— and at the end of 50 more, from 4 to 5°. In thefe places, Dr. *Halley* only makes 1°—— or 1°——½ variation, and in lieu of 50 leagues, makes 80 leagues between each degree of variation.

Having paffed the line he bore fouth-wefterly as far as 9°—— of fouth lat. and 356°—15'— long. where he alfo found the variation change a degree in 50 leag. diminifhing at this rate from 5 to 4, from 4 to 3, from 3 to 2, and from 2 to 1 ; fo that at the end of about 250 leagues, there was no variation at all, and 50 leagues further, the variation was 1°—— north-eaft ; whereas hitherto it had been north-weft ; fo that the place where the fhip cut the line of no variation

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is, by their account, 100 leagues more west-ward than laid down by Dr. *Halley.*

Proceeding on towards the island of *Afcenfion,* at 20 leagues north-eaft of that island, M. *Dau-mas* found the variation 6°——; where Dr. *Halley* alfo makes it 6°————north-eaft.

Paffing hence to the island *Grande,* on the coaft of *Brazil,* M. *Brunet* relates, that the va-riation was found 11°————$\frac{2}{3}$, which is much the fame as in Dr. *Halley*'s chart.

From hence paffing to the ftraits of *Magellan,* he found the variation at 12°————north-eaft, where Dr. *Halley* makes it 12 $\frac{1}{2}$, 13 where he makes it 13 $\frac{1}{2}$, 16 where he makes it 16 $\frac{1}{2}$, 17 where he makes 18 $\frac{1}{2}$, 18 where he makes it 19, 19 where he makes 19 $\frac{1}{2}$, and 19 $\frac{1}{2}$ where he makes it 20°. ————All thefe obfervations I relate, becaufe they confirm one another ; with regard to which it may alfo be obferved, that the laft obfervation given by M. *Brunet,* was made in 40°——30'—— fouth lat. and that the fhip having failed 60 leagues under the fame parallel, he found the fame va-riation in three fucceffive obfervations at this la-titude, which perfectly agrees with the inclina-tion of the lines of variation given by Dr. *Hal-ley* about this place, they being inclined from eaft to weft, for the fpace of 50 or 60 leagues, and thence turning infenfibly towards the fouth-weft in form of an *ellipfis,* as far as the ftraits of *Magellan.*

Arriving in *December* 1706, at the latitude of 57°——10' and 60 leagues fouth-weftward of the ftraits of *Le Maire,* M. *Brunet* relates, that the variation was found 26°————north-eaft, and that the fame variation continued for the fpace of 40 leagues. Here our comparifon ceafes with Dr.

*Halley*'s

*Halley*'s chart, which does not give the variations in this sea.

Having doubled *Cape Horn*, to the south of *Terra del Fuego*, they arrived at the city *Conception*, on the coasts of *Chili*, where M. *Brunet* found the variation 9°—30'. Hence they proceeded for *Valparaise*, where they found the variation 8°—. At *Pisco* and *Canete* 6°——½, and at *Callao*, the port of *Lima*, 6°——All which observations agree within half a degree to those made on the *Maurepas*, published by M. *Cassini*, who in examining the course of that vessel observes, that as their latitude increased, the variation increased ; to which, from these observations of Mess. *Brunet* and *Daumas*, we may add, that in the same parallel, the further they proceeded from the shores westward, the variation diminished.

Thus at 44°———45' lat. and about thirty leagues distance from the coasts of *Chili*, they found 12°—variation, and in the same parallel at 120 leagues distance from the coast found only 7°.—

Between 40 and 41° — lat. at 10 leagues distance from the shore, they found 9°— variation, and only 6°—— at 130 leagues distance from the same shore.

Between 30 and 31°—— lat. and 60 leagues from the shore, they found 7°——— variation, and only 5 at 220 leagues.

The same ship departing from the *Conception* in *December*, 1707, doubled *Cape Horn* a second time, came to anchor in the river *Gallegua*, not far from the straits of *Magellan*, and from whence set sail for the *Cape of Good Hope* ; being, I think,

the firſt ſhip that made this courſe, ſo that its obſervations are the more valuable.

At their departure from the river *Gallegua*, they found the variation 23°—north-eaſt.

At 60 leagues from this place, they found it 22°—north-eaſt.

At 30 leagues further 20°———
At 150 leagues further 18°———
At 110 leagues further 16°———
At 150 leagues further 14°———
At 60 leagues further 13°———
At 50 leagues further 12°———
At 20 leagues further 11°———
At 30 leagues beyond this 10°———
At 20 leagues further 8°———
At 100 leagues beyond this only 4°———

And, laſtly, at 120 leagues further the variation was nothing.

All theſe variations are north-eaſterly to the place where there was found no variation. — The following ones are north-weſterly.

At 60 leagues further eaſtward, they found the variation 2°———north-weſt.

At 80 leagues further 4°———
At 60 leagues beyond this 7°———
At 140 leagues further 9°—½.

And, laſtly, at 60 leagues further, near the *Cape of Good Hope*, where they arrived in *Mar.* 1708, the variation was found 8°———

Now, according to Dr. *Halley*'s chart, the variation ſhould have increaſed from the place of their departure in the ſpace of 240 leagues from 20 to 23°— and in the reſt of their paſſage ſhould have diminiſhed about 1° for every 2°— of longitude, as far as the line of no variation, and

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from hence to the *Cape*, have increafed north-weftward, in the fame proportion as it had diminifhed north-eaftward ; but it appears on the contrary, from thefe obfervations, that the greateft variation found by them in the whole paffage, was in this place of their departure, where it was found 23°. ———— It alfo appears, that when it did diminifh, this diminution did not always proceed in the fame proportion as expreffed in Dr. *Halley*'s chart ; but that during the firft 500 leagues, the variation diminifhed a degree for 4°———— of longitude; after which they found it diminifhed another degree, in the fpace of a degree and half of longitude ; but henceforward the variations altered, as in Dr. *Halley*'s chart, *viz.* 1°—— for 2 in longitude.

It alfo appears from thefe obfervations, that from the year 1700, the epocha of Dr. *Halley*'s chart to the year 1709, the line of no variation fhifted 50 leagues weftward in 35°———— of fouthern latitude. We have already obferved, that in the latitude of 22°——— it had fhifted 120 leagues, according to the obfervations of M. *Bigot*, that for the 7°———fouth, he found it 100 leagues more weftward ; and that at the latitude of 28°———north, he alfo found it more wefterly than Dr. *Halley* makes it at that latitude ; fo that the motion of this line weftward is confirmed by many obfervations.

Proceeding from the *Cape of Good Hope* eaftwards, they found the variation always increafe to the diftance of 530 leagues from the *Cape* ; and in 33°—½ fouth latitude, where they found the variation 24°—½ north-weft, which is the greateft they obferved in the *Indian* fea. Hence to

the

the iſland *Bourbon*, and to *Pondichery* and *Mer-guy*, they found it leſs and leſs every day, as at their return they found it every day greater and greater to the ſame term, and about the ſame diſtance from the *Cape*. Dr. *Halley* only differs about half a degree from theſe obſervations.

The other obſervations made by theſe perſons in the *Indian* ſea, differ but little from thoſe related by M. *Caſſini*, ſo that we content ourſelves to refer to them.

Theſe are all the obſervations of the variation that the journals afforded, I ſhall ſubjoin ſome remarks made from the ſame journals for the correction, and improvement of ſea charts, eſpecially thoſe of *Pieter Goos*, which might one day be rendered much more uſeful to navigators, were a great number of ſuch journals made.

The firſt pilot of the *Royal-dauphine* took notice of the *Salvages*, which are certain dangerous iſlands to the north of the *Canaries*, whoſe ſituation therefore cannot be too exactly known by the pilots ; he found them very ill laid down on the ſea charts, being placed too much eaſtward ; with regard to the iſland of *Porto Santo*, at a league and half ſouth-weſt of theſe iſlands, he found the latitude 30°— Their number he obſerves is two, whereof the more northern one is the greater, from whence a chain of rocks extends for about three leagues ſouth-weſtward ; at the end whereof is a little round iſland, with ſome flat ground, where the ſea beats extremely.

The firſt pilot of the *St. Louis*, in his paſſage to the iſland of *Aſcention*, found it placed by *Pieter Goos*, half a degree too much northward,

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its latitude he obferved to be 20°——2 2′, inftead of 19°——52′, as that author makes it.

The fhip *Louis* we have already noted to have been the firft which failed from the ftraits of *Magellan* directly to the *Cape of Good Hope*. At its departure from the river *Galleguas*, it made eaft-north-eaft the fpace of 1350 leagues, which brought it to that *Cape*; but in the way at 36°——54′ latitude, and 353°——10′ longitude, by the reckoning, they difcovered an ifland about a league diftance, and foon after two others, 3 or 4 leagues to the fouth-weft; and, laftly, a 4th, to the north-weft, which furprized them very much, they being 300 leagues from land.

M. *Brunet* took thefe iflands to refemble thofe of *Triftan de Cunha*, which he had feen in his voyage to *China* on board the *Amphitrite*; but M. *Hebert*, and the firft pilot were of a contrary opinion, by reafon, according to the reckoning, they had yet only made 750 leagues from the river *de Galleguas*, whereas *Pieter Goos*, placing thofe iflands in 12°—— of longitude, makes them 1050 leagues from the faid river; and they were further confirmed in this opinion upon their arrival at the *Cape of Good Hope*, where by the reckoning they found 35°—— of longitude, between thofe iflands and the *Cape* in lieu of 26°—— according to the chart, which makes a difference of 150 leagues in this parallel. Upon this they made no difficulty of accounting them a new difcovery, and gave them the name of the *Hebert* iflands, or the new iflands of *Triftan de Cunha*.

Yet to me the opinion of M. *Brunet*, who took them for the very iflands of *Triftan de*

*Cunha,* appears much more probable. 'Tis no new thing for pilots to make new difcoveries, and impofe new names upon iflands known long before them, of which we have an inftance in the ifland of *St. Helena,* which having been placed by the firft navigators more wefterly than it really is, was afterwards found by others more eafterly than it had been laid down in the charts, which made them take it for a new ifland in the fame latitude, but a different longitude from the firft, and accordingly they gave it the name of the new ifland *St. Helena,* and inferted it in the fea charts ; in moft whereof, particularly thofe of *Pieter Goos,* it is ftill found fo. The ifland *St. Apollonia* in the *Indian* fea is the fame with the ifland of *Bourbon* ; and the *Ifle of Dogs* in the *South-Sea,* found by *Le Maire* in 1616, is the fame with the ifland of *Tiburones,* difcovered by *Magellan* in 1520; fo in all likelihood the new iflands difcovered by our navigators, are no other than thofe of *Triftan de Cunha.* For Dr. *Halley,* who was at the iflands of *Trif- tan de Cunha* in 1700, lays them down in his chart at the fame diftance which the reckoning of thefe perfons required.; tho' the chart of *Pieter Goos* makes them 150 leagues off ; and as to the diftance of thefe iflands from the river *de Gale- guas,* which by their reckoning was 750 leagues, 'tis true, Dr. *Halley* makes it 170 leagues more ; but then the longitude, in which that author places the mouth of this river, muft be dimi- nifhed by 10°——— as I have done in my map of *South America,* from feveral affured obfervations, and among others, that of an eclipfe on the 13th of *Mar.* 1653, obferved by father *Mafcardi* in the valley of *Bucalene* in *Chili* ; for by this ob-
fervation,

ervation, compared with others made at *Paris*, *Bucalene* is 72°——½ weftward of *Paris*, and confequently the river of *Galeguas*, whofe diftance from *Bucalene* is known, fhould only be 68° from *Paris*; add, that by the obfervations of father *Fontaney* it appears, that the *Cape of Good Hope* is 17°——45' more eaftward than *Paris*; fo that the diftance between that *Cape* and the river of *Galeguas*, muft be 84°——25, which comes within a degree and half of the reckoning of the *St. Louis*.

XI.

XI. *Reflections on the observations of the flux and reflux of the sea, made at* Havre de Grace, *by M.* Boissaye du Bocage, *professor of hydrography, during the years* 1701 *and* 1702. *by M.* Cassini, *jun* *. *translated by Mr.* Chambers.

The place chosen by M. *Du Bocage* for his observations, was a part of the port best under shelter, here he erected a plank 10 feet ½ long against the wall of the port, and divided it into inches, whereon he observed the tides which happened in the day-time, having no conveniency for taking those of the night.

He observes, that the sea, while it rose, drove to the south-east and to the north-west, when it fell ; that the wind which traversed the road was west-north-west, and at the entrance of the port lay in the direction of the west-south-west wind.

The journal of his observations begins on the 9th of *April*, 1701, and ends on the 26th of *May*, 1702.

At first he contented himself with marking day by day the part of the plank to which the flood rose, with the winds that blew both in the ebb and flow ; but two months after, beginning on the 10th of *June* he observed the hours and minutes of high water, which he continued till the end of his observations, excepting a little interval between the 11th and 28th of *Nov.* during which he was obliged to have his watch mended.

Upon examining the times of high water observed at *Havre de Grace*, we find that on the days of full moon, the flood usually happens a

* Aug. 13, 1710.

little

little after 9 in the morning. The earlieſt high water was on the 19th of *July*, the day of full moon, when it happened at 39′ paſt 8 in the morning ; and the lateſt was on the 12th of *April*, at 39′ paſt 9 ; which gives a variation of an hour, in the time of the tides for the day of full moon.

Suppoſing the time of high water at *Havre de Grace* on the day of full moon to be at 9′—26′ in the morning, when the time of full moon coincides with that of high water, and that an allowance is made of 2′ for each hour, that the time of full moon comes, before or after the time of high water, as already ſettled at *Dunkirk*, we we ſhall find leſs variations in the times of high water obſerved at *Havre de Grace*. For an inſtance, on the 19th of *July*, 1701, high water happened at 8′——39′ in the morning, which is the greateſt anticipation M. *Du Bocage* obſerved. Now full moon is marked that day in the almanack at 11′—50′ in the evening, the difference between 9′—26′ in the morning, and 11ʰ—50′ in the evening, is 14ʰ—24′ ; to which at the rate of 2′ *per* hour anſwer 29′, which ſubſtracted from 9ʰ—26′, give 8′——57′ for the time of high water, within 18′ of what was obſerved at *Havre de Grace*.

'Tis obſerved, that the 19th of *July*, 1701, is the ſame day on which the greateſt acceleration of high water at full moon was obſerved at *Dunkirk*.

As to the lateſt high water on the days of full moon, it was found on the 12th of *April* at 9ʰ——39′, the full moon happening at 0′——13′ in the evening ; ſo that here ſhould have been an anticipation of 6′, which ſubtracted from 9 ——26′, gives the time of high water on the

12th of *April*, 1701, at 9$^h$——20′ within 19′ of what was obferved.

Upon a like examination of the obfervations of high water made at *Havre* in the time of new moon, it appears at firft fight, that the floods in the new moons happen 12 or 13′ later, than in the full moons ; and yet allowing for the acceleration or retardation of 2′ *per* hour above-mentioned, and fuppofing the time of high water in the new moons at *Havre*, to be at 9$^h$—26′ in the morning, as in the full moons, the obfervations of the full moons will be eafily reconciled to thofe of the new moons, excepting two, whofe regularity was extraordinary, there being an anticipation of feveral minutes in the time of high water, between one day and another, in lieu of the ufual retardation.————The new moon, wherein high water came the earlieft, was on the 29th of *Nov.* 1701, when it was flood, at 8$^h$————56′ in the morning, and the new moon is marked in the almanack at 10$^h$————11′ in the evening of that day, the difference between 9$^h$————26′ in the morning, and 10$^h$————11′ in the evening, is 12$^h$————45′ ; to which anfwer 25′ ½, which fubftracted from 9$^h$————26′, by reafon the new moon happened on the evening, gives the time of high water at 9$^h$————0′ ½, within 4′ of what was obferved.

. It is obfervable, that this new moon was the fame as that wherein the greateft acceleration of high water was found at *Dunkirk*, which we have attributed to the time of new moon, which happened at 10$^h$——11′ in the evening.

The new moon, wherein it was found flood the lateft, was that of *Auguft* the 4th, 1701, high water on that day was found at 9$^h$————48′ in the morning, and the new moon is fixed by

the

the almanack to 10$^h$——15′ in the fame morning. Now by the rule laid down, high water would have happened at 9°——24′ in the morning, within 24′ of what was obferved ; but it is obfervable, that from the 4th to the 5th of *Aug.* there was an anticipation of 11′ in time of high water, in lieu of a retardation ; fo that nothing can be built upon this obfervation, nor even upon that of the 2d of *September*, 1701, the day of new moon, when high water was obferved at 9$^h$——46′ in the morning, there being an anticipation here likewife of 20′ from the 2d to the 3d of *Sept.*

As to the higheft tides at *Havre de Grace*, they do not ufually fall upon the day of new and full moon, but one or two days after, as has been obferved at *Dunkirk*; and as they have likewife been found not only in other parts of the channel, but on the coafts of *Africa* and *America*.

The higheft tide obferved at *Havre* was on the 15th of *Feb.* 1702, when the flood rofe 18 feet, 3 inch. 3 days after full moon, which had happened on the 12th of the fame month at 3$^h$—— 2′ in the evening, the wind was then very ftrong, and blew at fouth-fouth-weft, which confpiring with the tide, and driving it towards the port, might have contributed to its extraordinary elevation that day.

As to the tides near the equinoxes at new and full moons, fome of them are higher, and others lower fo that it can be no rule at *Havre*, that the equinoctial tides are the higheft.

What is further remarkable is, that the heights of the tides keep exactly pace with the different diftances of the moon from the earth in all ftates, whether near the equinoxes, or at any diftance therefrom, as has likewife been obferved at *Dunkirk.*

*kirk.*———For an inftance on the 10th of *April*, 1701, two days after the new moon fucceeding the vernal equinox, the height of the water was found 15 feet, 6 inches, 6 lines, which is lower by 1 foot, 3 inches $\frac{1}{2}$, than on the 10th of *May* ; two days after the next full moon, which yet was further from the equinox, agreeably to the diftance of the moon, which was farther from the the earth on the 8th of *April*, the day of new moon ; than on the 24th of *April*, the day of full moon ; fo on the 19th of *September*, 1701, two days after the full moon next the autumnal equinox, the height of flood was found 15 feet, 11 inches, which was lower by 1 foot, 10 inches, than on the 4th of *October* ; two days after next new moon, when the water was found 17 feet, 9 inches, high, one of the higheft tides that had been known ; and again on the 16th of next *October*, the day of full moon high water was obferved 16 feet, 3 inches, which was lower than on the 4th of *October* ; and on the 1ft of *November* it was found 16 feet, 11 inches, which was higher than on the 4th of *October*.

Thefe different heights of the tides cannot be owing to the different diftances of the fun from the equinoxes; fince the water was lower near the equinox, than in the other obfervations further from it ; but they agree perfectly with the different diftances of the moon from the earth, at the time of new and full moons.————For on the 17th of *September*, the moon was further from the earth than in the other obfervations on the 2d of *October* ; it was nearer on the 16th of the fame month ; it was further diftant than on the 4th, but lefs on the 17th of *September* ; and on the 31ft of *October* it was nearer than on the 17th, but further than on the 4th of *October*.

It would be tedious to rehearfe all the relations between the obfervations, and the diftances of the moon from the earth ; it may fuffice to obferve, that on the 15th of *March*, 1702, high water was found 17 feet, 1 inch ½, whereas on the 30th of *March*, at an equal diftance from the equinox, it was only found 15 feet, 8 inches; and that on the 15th of *April* it was obferved 17 feet, 6 inches ¾ ; and on the 28th of *April*, 15 feet, 2 inches, all which agrees with the different diftances of the moon, which was nearer the earth on the 15th of *April*; and further off on the 28th of the fame month, than in any other preceding obfervation, as may be feen in the following table :

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## A TABLE of the time and height of the tides,

| Days and Hours of the new and full moons. | | | Time of high water observed. | | Time of high water calculated. | | Height of the sea | | |
|---|---|---|---|---|---|---|---|---|---|
| 1701. | H. | M. | H. | M. | H. | M. | F. | In. | L. |
| ● 8 Apr. | 10 | 54 M. | | | | | | | |
| ☉ 22 Apr. | 5 | 16 E. | | | | | 16 | 3 | |
| ● 8 May, | 1 | 42 M. | | | | | 15 | 2 | |
| ☉ 22 May, | 2 | 18 M. | | | | | 15 | 11 | |
| ● 6 June, | 2 | 28 E. | | | 9 | 16 | 14 | 11 | 6 |
| ☉ 20 June, | 0 | 26 E. | 9 | 1 | 9 | 20 | 15 | 4 | |
| ● 6 July, | 0 | 58 M. | 9 | 25 | 9 | 43 | 15 | 0 | 3 |
| ☉ 19 July, | 11 | 50 E. | 8 | 39 | 8 | 57 | 14 | 6 | 9 |
| ● 4 Aug. | 10 | 15 M. | 9 | 48 | 9 | 24 | 15 | 8 | 6 |
| ☉ 18 Aug. | 2 | 6 E. | 9 | 6 | 9 | 16 ½ | 15 | 4 | |
| ● 2 Sept. | 6 | 5 E. | 9 | 46 | 9 | 8 ½ | 15 | 11 | 6 |
| ☉ 17 Sept. | 5 | 56 M. | 9 | 30 | 9 | 34 | 15 | 4 | |
| ● 2 Oct. | 2 | 20 M. | 9 | 33 | 9 | 40 | 17 | 2 | |
| ☉ 16 Oct. | 11 | 24 E. | 9 | 12 | 8 | 58 | 16 | 3 | |
| ● 31 Oct. | 11 | 24 M. | 9 | 34 | 9 | 22 | 16 | 7 | 6 |
| ☉ 15 Nov. | 5 | 4 E. | | | 9 | 11 | 15 | 10 | |
| ● 29 Nov. | 10 | 11 E. | 8 | 56 | 9 | 0 | 17 | 0 | |
| ☉ 15 Dec. | 10 | 16 M. | 9 | 30 | 9 | 24 | 15 | 4 | 8 |
| ● 29 Dec. | 10 | 47 M. | 8 | 59 | 9 | 23 | 15 | 11 | 9 |
| 1702. | | | | | | | | | |
| ☉ 14 Jan. | 1 | 12 M. | 9 | 25 | 9 | 43 | 16 | 5 | 8 |
| ● 28 Jan. | 1 | 57 M. | 9 | 45 | 9 | 43 | 16 | 1 | 6 |
| ☉ 12 Feb. | 3 | 2 E. | 9 | 6 | 9 | 15 | 15 | 9 | |
| ● 26 Feb. | 6 | 15 E. | 9 | 25 | 9 | 8 | 15 | 6 | 4 |
| ☉ 14 Mar. | 1 | 48 M. | 9 | 34 | 9 | 41 ½ | 16 | 7 | |
| ● 28 Mar. | 11 | 7 M | 9 | 38 ½ | 9 | 23 | 15 | 3 | |
| ☉ 12 Apr. | 0 | 13 E. | 9 | 39 | 9 | 20 | 16 | 9 | |
| ● 27 Apr. | 5 | 49 M. | 9 | 40 | 9 | 33 | 15 | 0 | 9 |
| ☉ 11 May, | 4 | 59 E. | 9 | 23 | 9 | 11 | 16 | 3 | 3 |
| ● 26 May, | 8 | 27 E. | 9 | 25 | 9 | 4 | 14 | 1 | 6 |

## *in the new and full moon at* Havre de Grace.

| Diftance of the fun from the apogee of the moon. | Diftance of the moon from the earth in conjunct. and oppof. | Day of the higheft tide. | Height of the fea. | | |
|---|---|---|---|---|---|
| D. M. S. | | | F. In. L. | | |
| 1  0  21 | 105589 | 10 April. | 15 | 6 | 6 |
| | | 24 April. | 16 | 10 | |
| | | 9, 10 May. | 15 | 6 | 3 |
| | | 23 May. | 16 | 0 | 6 |
| | | 8, 9 June. | 15 | 7 | |
| 3  2  33 | 99713 | 21 June. | 15 | 5 | 9 |
| 3  16  2 | 98372 | 7 July. | 15 | 3 | 3 |
| | | 22 July. | 15 | 0 | 3 |
| | | 7 Auguft. | 16 | 10 | |
| | | 19 Auguft. | 15 | 11 | 6 |
| | | 4 September. | 17 | 6 | |
| 5  18  10 | 106340 | 19 September. | 15 | 11 | |
| 6  1  13 | 93469 | 4 October. | 17 | 9 | |
| | | 16, 17 October. | 16 | 3 | |
| | | 1 November. | 16 | 11 | |
| | | 16 November. | 15 | 11 | |
| | | 30 November. | 17 | 10 | |
| | | 18 December. | 15 | 6 | |
| | | 30 December. | 16 | 5 | 6 |
| | | 1702. | | | |
| 9  4  37 | 100013 | 14 January. | 16 | 5 | 8 |
| 9  17  19 | 100477 | 29 January. | 16 | 4 | 9 |
| | | 15 February. | 18 | 3 | |
| | | 28 February. | 17 | 2 | |
| | | 15 March. | 17 | 1 | 6 |
| | | 30 March | 15 | 8 | |
| 11  22  55 | 93519 | 15 April. | 17 | 6 | 9 |
| 0  5  52 | 106496 | 28 April. | 15 | 2 | 3 |
| | | 12 May. | 17 | 0 | |

If now we examine the time of high water observed in the quadratures, we shall find it happen at *Havre* 30′ past 2 in the evening.

Among the 23 observations made hereof, the flood which come the earliest, was on the 6th of *March*, 1702, at 1ʰ—55′ in the evening, and the latest, on the 5th of *May*, at 3ʰ—30′.

To frame some rule of this variation, we suppose the mean time of high water in the quadratures at *Havre*, to be at 2ʰ—40′ in the evening, and add or subtract from this time 2′ per hour, for the time which the quadrature noted in the almanack anticipates, or comes behind the mean time of high-water, as fixed at 2ʰ—40′ in the evening.

For an instance on the 6th of *March*, 1702, high water was observed at 1ʰ—55′ in the evening; the first quadrature is noted in the almanack for that day at 10ʰ—24′ in the evening, the difference between 2ʰ 40′ and 10ʰ—24′ in the evening, is 7ʰ—44′, to which, at the rate of 2′ per hour, answer 15′ ½, which substracted from 2ʰ—40′, gives 2ʰ—24′ ½, for the time of high water within 29′ ½ of what was observed.———So on the 5th of *May*, 1702, the day when the tide came latest in the quadratures, high water was observed at 3ʰ———30′, the first quadrature is fixed in the almanack at 1ʰ———59′, that day in the morning the difference between 1ʰ———59′ in the morning, and 2ʰ———40′ in the evening, is 12ʰ—41′, to which at 2 per hour, answer 25′, which added to 2ʰ———40′, give 3ʰ———5′, for the time of high water within 25′ of what was observed.

The time of high water would have been found more exactly in these two observations, if in lieu of 2′ for each hour of anticipation, or retardation,

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we had taken 3′, which agrees nearer with the retardation of the tides from 1 day to another, about the quadratures which usually exceeds an hour.

High water happening at *Havre* in the new and full moons at 9ʰ—26′ in the morning, and in the quadratures at 2ʰ—40′, we have 5ʰ—14′ for the interval between the times of high water from the new and full moons to the quadratures.—— This interval was found at *Dunkirk* 5ʰ—12′, which shews the great conformity between the times of the tides in those 2 ports.

As to the least tides observed at *Havre de Grace*, they do not usually happen in the 2 quadratures, but 1 or 2 days after the first or last quarter.— The lowest tide was found on the 8th of *March*, 1702, two days after the 1st quadrature, the water on that day rising only 9 feet, 8 inches, 4 lines; and the highest tide, as already mentioned, happened the 15th of *February*, and rose 18 feet, 3 inches; so that there is a difference of 8 feet, 7 inches, between the highest and lowest tides at *Havre*; whereas the difference was only found 7 feet at *Dunkirk*.

By comparing the different heights of the tides observed at *Havre*, about the quadratures, we likewise find them bear a near relation to the different distances of the moon from the earth, as will appear from the following table.———— Where it is observable, that on the 22d of *January*, 1702, the moon being in Perigeo about the quadratures, the height of the flood was found 13 feet, 2 inches, 8 lines, which is less only by 2 feet, than on the 28th of *April*, 1702, two days after the new moon, which was then in apogeo.

*A* TABLE *of the time and height of the*

| Days and hours of the quadratures. | | | Time of h water bferved. | Time of high water calculated. | Heigl t of the fca. |
|---|---|---|---|---|---|
| | | | H. M. | H. M. | F. In. L. |
| | | 1701. | | | |
| 1 □ | 16 Apr. | 2  8 M. | | 3  6 | 11  2  6 |
| 3 □ | 29 Apr. | 10 25 E. | | 2 24 | 12  2  6 |
| 1 □ | 15 May | 9  6 M. | | 2 51 | 13  0  6 |
| 3 □ | 29 May | 3 38 E. | | 2 38 | 12  3  0 |
| 1 □ | 13 June | 2 26 E. | 3  1 S. | 2 40 ½ | 13  6 |
| 3 □ | 28 June | 9 14 M. | 3 10 | 2 51 | 11  9 |
| 1 □ | 12 July | 7 17 E. | 2  5 | 2 31 | 13 11 |
| 3 □ | 28 July | 2 23 M | 3  0 | 3  5 | 11 10 |
| 1 □ | 11 Aug. | 1 14 M. | 2 50 | 3  7 | 13 11  6 |
| 3 □ | 26 Aug. | 6  6 E. | 2  0 ½ | 2 33 | 12  4  3 |
| 1 □ | 9 Sept. | 9 20 M. | 2 42 | 2 51 | 13  7  8 |
| 3 □ | 25 Sept. | 7 58 M. | 2 24 | 2 53 ½ | 11  3  6 |
| 1 □ | 8 Oct. | 8 47 E. | 2 29 | 2 28 | 13  6  4 |
| 3 □ | 24 Oct. | 7 50 E. | 2 30 | 2 30 | 11  7  9 |
| 1 □ | 7 Nov. | 0 17 E. | 2 51 | 2 45 | 12  5 |
| 3 □ | 23 Nov. | 6  0 M. | | 2 58 | 12  6 |
| 1 □ | 7 Dec. | 7 31 M | 2 51 | 2 54 ½ | 12  4 |
| 3 □ | 22 Dec. | 8 48 E. | 2 39 | 2 28 | 13  6 |
| | | 1702. | | | |
| 1 □ | 6 Jan. | 5 47 M. | 3  4 | 2 58 | 14 33 |
| 3 □ | 20 Jan. | 10 41 E. | 2 13 | 2 24 | 15  3⅓ |
| 1 □ | 5 Feb. | 2 34 M. | 2 36 | 3  4 | 12  5  3 |
| 3 □ | 19 Feb. | 6 33 M. | 2 24 | 2 56 ½ | 13  4 |
| 1 □ | 6 Mar. | 10 24 E. | 1 55 | 2 24 ½ | 11  3  6 |
| 3 □ | 20 Mar. | 3 47 E. | 2 46 | 2 38 | 13  4  9 |
| 1 □ | 5 Apr. | 2  9 F. | 2 12 | 2 41 | 11  8 |
| 3 □ | 19 Apr. | 3 39 M. | 3 14 | 3  2 | 12  2  9 |
| 1 □ | 5 May | 1 59 M. | 3 30 | 3  6 | 11  5 |
| 3 □ | 18 May | 4  4 E. | 3 10 ½ | 2 37 | 12  2  8 |

*tides in the quadratures at* Havre de Grace.

| Diftance of the fun from the apogee of the moon. | | | Diftance of the moon from the earth in the quadratures. | Day of the loweft tide. | Height of the fea. | | |
|---|---|---|---|---|---|---|---|
| D. | M. | S. | | | F. | In. | L. |
| | | | | 17 April. | 10 | 10 | |
| | | | | 1 May. | 10 | 10 | 6 |
| | | | | 16 May. | 12 | 7 | 6 |
| | | | | 30 May. | 11 | 4 | |
| 2 | 26 | 40 | 97615 | 14 June. | 13 | 0 | |
| 3 | 9 | 17 | 106250 | 29 June. | 11 | 3 | |
| | | | | 14 July. | 12 | 11 | |
| | | | | 29 July. | 11 | 6 | |
| | | | | 13 Auguft. | 12 | 7 | |
| | | | | 28 Auguft. | 11 | 8 | 6 |
| | | | | 10 September. | 11 | 8 | 9 |
| 5 | 25 | 8 | 102165 | 26 September. | 10 | 7 | |
| 6 | 6 | 30 | 102275 | 10 October. | 11 | 2 | 3 |
| | | | | 25 October. | 10 | 4 | 8 |
| | | | | 8 November. | 11 | 5 | |
| | | | | 23 November. | 12 | 6 | |
| 8 | 28 | 20 | 106425 | | | | |
| 9 | 10 | 2 | 97717 | 22 January. | 13 | 2 | 8 |
| | | | | 6 February. | 10 | 10 | |
| | | | | 21 February. | 12 | 7 | 9 |
| | | | | 8 March | 9 | 8 | 4 |
| | | | | 22 March. | 10 | 10 | 4 |
| | | | | 6 April. | 10 | 6 | 3 |
| 11 | 28 | 56 | 101725 | 20 April. | 11 | 6 | 3 |
| 0 | 12 | 40 | 100856 | 7 May. | 11 | 6 | |
| | | | | 20, 21 May. | 11 | 4 | |

After

After afcertaining the retardation of the tides in the new and full moons, and quadratures, we examined all the obfervations at *Havre de Grace*, and found that the mean retardation of the tide from one day to another was almoft perfectly the fame with that obferved at *Dunkirk*; fo that the fame rule may ferve for finding the time of high water every day in the year, in both thofe ports. We have alfo drawn up a table of thefe retardations like that given for *Dunkirk*, by means whereof it may be found, whether the ebbing and flowing follow the fame rule in other ports.———In order hereto, the mean time of the tide muft be fettled in each port, for the days of new and full moon, and quadratures for the reft, the rules will obtain already prefcribed.

The tides in this table are marked for every 12 hours after full moon, for the conveniency of finding the morning and evening tides.

9h 26′ M. mean time of the high tide at *Havre,* the day of the new and full moons. | 2h 40′ E. mean time of the high tide at *Havre,* the days of the quadratures.

*A TABLE of the retardation of the tides.*

| Days and hours after new or full moon. | Retardation of the tides. | Dif. | Days and hours after first or last quarter. | Retardation of the tides. | Dif. |
|---|---|---|---|---|---|
| D. H. | H. M. | M. | D. H. | D. H. | M. |
| 0 0 | 0 0 | | 0 0 | 0 0 | |
| | | 26 | | | 32 |
| 0 12 | 0 26 | | 0 12 | 0 32 | |
| | | 24 | | | 36 |
| 1 0 | 0 50 | | 1 0 | 1 8 | |
| | | 21 | | | 41 |
| 1 12 | 1 11 | | 1 12 | 1 49 | |
| | | 19 | | | 43 |
| 2 0 | 1 30 | | 2 0 | 2 32 | |
| | | 18 | | | 39 |
| 2 12 | 1 48 | | 2 12 | 3 11 | |
| | | 18 | | | 33 |
| 3 0 | 2 6 | | 3 0 | 3 44 | |
| | | 18 | | | 30 |
| 3 12 | 2 24 | | 3 12 | 4 14 | |
| | | 18 | | | 26 |
| 4 0 | 2 42 | | 4 0 | 4 40 | |
| | | 19 | | | 24 |
| 4 12 | 3 1 | | 4 12 | 5 4 | |
| | | 20 | | | 24 |
| 5 0 | 3 21 | | 5 0 | 5 28 | |
| | | 20 | | | 22 |
| 5 12 | 3 41 | | 5 12 | 5 50 | |
| | | 21 | | | 22 |
| 6 0 | 4 2 | | 6 0 | 6 12 | |
| | | 19 | | | 22 |
| 6 12 | 4 21 | | 6 12 | 6 34 | |
| | | 18 | | | 20 |
| 7 0 | 4 39 | | 7 0 | 6 54 | |

Rule first, To find the time of high water at *Havre,* for the days of new and full moon, and quadratures.

Look in the almanack for the time of new and full moon, and quadratures, and take the difference between this and the mean time of high water expreffed for the day of that phafis; doubling this difference, you will have the number of minutes to be added to the mean time of high water, if the phafis anticipate fuch mean time,

time, or be fubtracted from the time of the phafis, come behind the fame, the refult will be the true time of high water for the day of the given pha- fis, whether it be new or full moon, or one of the quadratures.

For an inftance, fuppofe the time of high water required for the day of new moon in *Jan.* 1702.

In the *Ephemeris* we find new moon marked on the 28th of *January* at $0^h$——57' in the morning, the difference between $0^h$——57' in the morning, and $9^h$——26' in the morning. The mean time of high water at new and full moons in this port is $8^h$——29', the double whereof, *viz.* 16'——58" is the number to be added to $9^h$——26'. The mean time of high-water at *Havre* on account of the full moon's anticipating the time of high water the fum, *viz.* $9^h$——43' is the true time of high water on the 28th of *January*, 1702. M. *du Bocage* obferved it $9^h$——45'.

Rule 2d. To find the time of high water at *Havre* for any given day.

Seek by the firft rule the time of high water, for the day of new and full moon, or quadrature, next preceding the given day ; to this add the re- tardation of the tide correfponding to the diffe- rence between the given day, and the day of the preceding phafis, the fum will be the time of high water for the day required.

For an inftance ; fuppofe the time of high wa- ter required for the firft of *Feb.* 1702.

By the *Ephemeris* the phafis immediately pre- ceding the firft of *February*, is new moon, which happens on the 28th of *January*, 1702, at $0^h$——57' in the morning ; four days before the day given, the time of high water that day is found in the preceding example to happen at 9——43 in the morning ; add to this $2^h$——42' for

the

the retardation correfponding to 4 days, the fum will be the time of high water on the given day, *viz.* 0ʰ—25′ in the evening. M. *du Bocage* found it that day at 0ʰ—15′ in the evening.

To find the time of high water immediately preceding, or following a time thus found, we muſt ſubtract or add the difference in the table correfponding to 12ʰ.————We may likewiſe apply to *Havre de Grace* the three laſt rules laid down for finding the higheſt and loweſt tides at *Dunkirk*.

As the interval between the new and full moons, and quadratures, there is from 6 days to 8, it follows, that when this interval is 6 days, the retardation of the tide between one day and another, muſt be greater than when the interval is 8 days.———— And hence for the greater accuracy, we have framed a table wherein the retardation of the tide is aſſigned according to thoſe different intervals.

## A TABLE *of the retardation of the tides.*

| Days and hours after the new or full moon. | | Interval between the day of the new or full moons, and the days of the quadratures. | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 6 Days. | | | 7 Days. | | | 8 Days. | | | |
| | | Retardation of the tides. | | | | | | | | | |
| D. | H. | H. | M. | D | H. | M. | D | H. | M. | D | |
| 0 | 0 | 0 | 0 | | 0 | 0 | | 0 | 0 | | |
| | | | | 29 | | | 27 | | | 25 | |
| | 12 | 0 | 29 | | 0 | 27 | | 0 | 25 | | |
| | | | | 25 | | | 24 | | | 22 | |
| 1 | 0 | 0 | 54 | | 0 | 51 | | 0 | 47 | | |
| | | | | 22 | | | 22 | | | 20 | |
| | 12 | 1 | 16 | | 1 | 13 | | 1 | 7 | | |
| | | | | 21 | | | 20 | | | 18 | |
| 2 | 0 | 1 | 37 | | 1 | 33 | | 1 | 25 | | |
| | | | | 21 | | | 20 | | | 17 | |
| | 12 | 1 | 58 | | 1 | 53 | | 1 | 42 | | |
| | | | | 20 | | | 19 | | | 16 | |
| 3 | 0 | 2 | 18 | | 2 | 12 | | 1 | 58 | | |
| | | | | 20 | | | 19 | | | 16 | |
| | 12 | 2 | 38 | | 2 | 31 | | 2 | 14 | | |
| | | | | 20 | | | 19 | | | 17 | |
| 4 | 0 | 2 | 58 | | 2 | 50 | | 2 | 31 | | |
| | | | | 22 | | | 20 | | | 19 | |
| | 12 | 3 | 20 | | 3 | 10 | | 2 | 50 | | |
| | | | | 23 | | | 22 | | | 20 | |
| 5 | 0 | 3 | 43 | | 3 | 32 | | 3 | 10 | | |
| | 12 | | | | 3 | 56 | 24 | 3 | 31 | 21 | |
| 6 | 0 | | | | 4 | 23 | 27 | 3 | 53 | 22 | |
| | 12 | | | | | | | 4 | 16 | 23 | |
| 7 | 0 | | | | | | | 4 | 39 | | |

| Days and hours after the quadra- tures. | Interval between the day of the new or full moons, and the day of the quadratures. | | | | | |
|---|---|---|---|---|---|---|
| | 6 Days. | | 7 Days. | | 8 Days. | |
| | Retardation of the tides. | | | | | |
| D. H. | H. M. | D. | H. M. | D. | H. M. | D |
| 0  0 | 0   0 | | 0   0 | | 0   0 | 29 |
| 0 12 | 0  37 | 37 | 0  31 | 31 | 0  29 | 29 |
| 1  0 | 1  16 | 39 | 1   6 | 35 | 1   2 | 33 |
| 1 12 | 1  58 | 42 | 1  47 | 41 | 1  42 | 40 |
| 2  0 | 2  42 | 44 | 2  32 | 45 | 2  26 | 44 |
| 2 12 | 3  26 | 44 | 3  16 | 44 | 3  11 | 45 |
| 3  0 | 4   3 | 37 | 3  50 | 34 | 3  45 | 34 |
| 3 12 | 4  32 | 26 | 4  17 | 27 | 4  12 | 27 |
| 4  0 | 4  58 | 20 | 4  42 | 25 | 4  37 | 25 |
| 4 12 | 5  22 | 24 | 5   5 | 23 | 4  59 | 22 |
| 5  0 | 5  46 | 24 | 5  28 | 23 | 5  22 | 23 |
| 5 12 | | | 5  53 | 25 | 5  47 | 25 |
| 6  0 | | | 6  18 | 25 | 6  14 | 27 |
| 6 12 | | | | | 6  41 | 27 |
| 7  0 | | | | | 7   8 | 27 |
| 7 12 | | | | | | |

To make ufe of this table, we find in the *Ephemeris* the numbers of days between the phafis immediately preceding, and that fucceding the given day, and take the retardation of the tide expreffed in the column under that number of days.

For an inftance; fuppofe the time of high water required for the 1ft of *February*, 1702, which is the inftance propofed under the 2d rule.——The *Phafis* next preceding the 1ft of *February*, is the new moon of the 28th of *January*; and the phafis following it, is the firft quadrature which happens on the 5th of *February*; the interval between thefe two phafes is 8 days: feek under the column

marked a-top with 8 days, the retardation anfwering to 4 days, which is 2 hours 31 ; add this to $9^h$—43' (the time of high water on the day of the preceding new moon found by the rule) and the fum will give the time of high water on the 1ft of *February*, 1702, at $0^h$—14' in the evening within a minute of what was actually obferved.

## XII. *Reflections on the obfervations of the tides made at* Breft, *and at* Bayonne, *by* M. Caffini, *the fon.* *

Having found, that the obfervations of the flux and reflux of the fea, made at *Dunkirk*, and *Havre de Grace* agree, fo that we may ufe the fame rules to find in both thefe ports the time of high water, for every day in the year, precifely enough. We have thought proper to examine if thefe rules agreed wi·h the obfervations upon the tides made at *Breft* and *Bayonne*, by Meff. *de la Hire* and *Picard*.

Thefe obfervations are related in the collection of the travels of the academy ; they were made at *Breft*, in *September* 1679, in the king's garden, which has a view to the port where the fea is commonly very calm.

Meff. *de la Hire* and *Picard* obferved there, from the 18th to the 28th of *September*, the time of high and low water. They did not wait to make their obfervations, till the tide was entirely high or low, becaufe then it remains too long in that ftate ; but they marked 2 diftant times before and after, in which they found it at a certain exact

* Aug. 16, 1710.

height

height, which lasted so little, that they made no difficulty to mark even to seconds. They afterwards took the middle of the time which passed between the corresponding observations.

In comparing at first the retardation of the tide, from the 18th to the 19th of *September*, they found it 48' equal to the mean motion of the moon. This retardation is afterwards a little less to the 26th of the same month; but from the 26th to the 28th, it is excessive, there having been from the 26th to the 27th, a retardation in the time of high water of $1^h$ 9' 45", and from the 27th to the 28th of $1^h$ 30' 30".

As the rules, that we have prescribed to find the time of high water at *Dunkirk* and *Havre de Grace*, require the knowing the days and hours of the new and full moons, and of the quadratures, we have examined the days of the moon, upon which these observations have been made, and have found in the *Connoissance des Temps* of 1679, that the moon was at the full on the 20th of *Sept.* at $7^h$ 48' in the morning; and in the last quarter, the 26th, at $7^h$ 4' at night. The 20th of *Sept.* the day of the full moon, the height of the tides was not observed there : but by the comparison of some observations that were made the preceding and following days, we see that it must have happened about 4 in the evening. We find afterwards, that the retardations of the tides was in diminishing, and pretty near agree with that which results from the rule ; but from the 26th to the 28th, the daily retardation was greater, as it ought to happen according to the rule. For the last quarter of the moon being the 26th, it ought to have a retardation from the 26th to the 27th of $1^h$ 8', and from the 27th to

the

the 28th, of 1ʰ 24′, within a few minutes of what was obſerved from the 26th to the 27th of 1ʰ 10′, and from the 27th to the 28th of 1ʰ 30′ 30″.

For the more eaſy comparing the rule with the obſervations, we have prepared the following table ; where there is marked in the firſt column, the day of the obſervation ; in the ſecond, the time of the high or low water, determined by the obſervation ; in the third and fourth, the time calculated according to the firſt and ſecond table ; and in the fifth and ſixth, the difference between the time obſerved, and the time calculated according to the firſt and ſecond table of the retardation of the tides.

*A*

## A TABLE *of the tides obferved at* Breft.

| 1679.<br>tember.<br>Days. | Time of the high or low-water obferved. | Time calculated. | | Diff. by the firft table. | Diff. by the fecond table. |
|---|---|---|---|---|---|
| | | By the firft table. | By the fecond table. | | |
| | H. M. S. | H. M. | H. M. | M. | M. |
| 18 | 2 25 30 E. H. W. | | | | |
| 19 | 3 13 30 E. H. W. | | | | |
| l moon 7 hours minutes, rn. 20 | | 4 2 E. | 4 2 | | |
| 21 | 10 29 30 M. L. W. | 10 40 | 10 43 | 10 $\frac{1}{2}$ | 13 $\frac{1}{2}$ |
| 22 | 11 41 45 E. L. W. | 11 41 | 11 49 | 0 $\frac{3}{4}$ | 7 $\frac{1}{4}$ |
| 24 | 0 25 30 M. L. W. | 0 17 | 0 30 | 8 $\frac{1}{2}$ | 4 $\frac{1}{2}$ |
| | 0 46 30 E L. W. | 0 35 | 0 50 | 11 $\frac{1}{2}$ | 3 $\frac{1}{2}$ |
| 25 | 1 12 30 M. L. W. | 0 53 | 1 11 | 19 $\frac{1}{2}$ | 1 $\frac{1}{2}$ |
| | 1 34 30 E. L. W. | 1 13 | 1 34 | 21 $\frac{1}{2}$ | 0 $\frac{1}{2}$ |
| uarter at hours 4/ ight. 26 | 1 56 40 M. L. W. | 1 53 | 1 53 | 3 $\frac{2}{3}$ | 3 $\frac{2}{3}$ |
| | 8 6 45 M. H. W. | 8 6 45 | 8 6, 45 | 0 | 0 |
| 27 | 3 38 30 M. L. W. | 2 57 | 2 54 | 41 $\frac{1}{2}$ | 44 $\frac{1}{2}$ |
| | 9 16 30 M. H. W. | 9 15 | 9 9 | 1 $\frac{1}{2}$ | 7 $\frac{1}{2}$ |
| | 10 9 30 E. H. W. | 9 56 | 9 49 | 13 $\frac{1}{2}$ | 20 $\frac{1}{2}$ |
| 28 | 10 47 0 M. H. W. | 10 39 | 10 33 | 8 | 14 |

## flections *upon the obfervations of the tides made at* Bayonne.

The obfervations upon the flux and reflux of the fea, re made at *Bayonne*, by Meff. *de la Hire* and *Picard*, the *Dour*, where the fea rifes confiderably.

They obferved in the fame manner, that they had done *Breft*, the preceding year, the time of high and low ter from the 12th of *Sept.* 1680, to the 4th of *Oct.* lowing.

To be able to compare the rule of the retar
dation of the tides with the obfervations, the
firft fought in the *Connoiſſance des Temps* of 168c
the days and hours of the *phaſes* of the moon, and
they found that the moon entered the laft quarte
the 15th of *September*, 1680, at 1$^h$ 4$'$ in the morn
ing; that the new moon following happened or
the 22d at 7$^h$ 30$'$ in the evening; and that th
firft quarter happened on the 30th of *September*
11$^h$ 4 in the evening.

It follows then from the rule that to begin fron
the 15th of *September*, the day of the laft quarter
there ought to have been a great retardation in th
tide from one day to another, during 3 or 4 days
that from the 22d of *September* the day of the new
moon to the 30th of *September*, the day of the
laft quarter, there had been an acceleration in the
tides, and that from the 30th of *September*, to the
4th of *October*, there ought to have been a retar-
dation in the tide, which entirely agrees with the
obfervations.

This conformity of the rule of the retardation
of the tides, with the obfervations, has given us
room to examine, whether they agree in all the
circumftances. For this is fuppofed, that the
high water happens at *Bayonne*, the day of the
new and full moon at 3$'$ 30$'$, the fame that is
marked in the *Connoiſſance des Temps*. The in-
terval between the time of the tides from the new
or full moon to the quadratures, being 5$^h$ 14, as
they found at *Havre*, we fhall have the time of
high water in the quadratures, at *Bayonne*, at 8$^h$
44$'$. Upon thefe *hypotheſes* they have calculated
by the rules prefcribed in the preceding memoir,
the time of the high or low water in the obfer-
vations made at *Bayonne*, fince the 15th of *Sept.*
and they have marked them in the table fubjoin-

3                                                    ed,

with the differences, which are for the moft part fo
all, that we could never have hoped to have been able
arrive at fo great an exactnefs.

## A table of the tides obferved at Bayonne.

| 1680. | Times of high and low water obſerved. | Time calculated. | | Diff. by the firſt table. | Diff. by the ſecond table. |
|---|---|---|---|---|---|
| | | By the firſt table. | By the ſecond table. | | |
| ſeptember. | | | | | |
| Days. | H. M. S. | H. M. | H. M. | M. | M. |
| 12 | 0 1 0 M. L. W. | | | | |
| | 0 24 30 E. L. W. | | | | |
| 13 | 0 43 0 M. L. W. | | | | |
| | 1 8 45 E. L. W. | | | | |
| 14 | 1 34 30 M. L. W. | | | | |
| | 2 0 30 E. L. W. | | | | |
| quarter, h. 4 m. | 2 35 30 M. L. W. | 2 38 | 2 39 | 2 | 3 |
| | 3 8 15 E. L. W. | 3 8 | 3 8 | 0 | 0 |
| 15 | 9 20 45 E. H. W | 9 23 | 9 23 | 2 | 2 |
| 16 | 3 44 0 M. L. W. | 3 39 | 3 38 | 5 | 6 |
| | 9 57 0 M. H. W. | 9 55 | 9 54 | 2 | 3 |
| | 10 40 30 E. H. W. | 10 31 | 10 29 | 9 | 11 |
| 18 | 0 13 30 E. H. W. | 0 34 | 0 37 | 20 | 23 |
| 19 | 1 14 0 M. H. W. | 1 7 | 1 13 | 7 | 1 |
| | 1 43 0 E. H. W. | 1 37 | 1 40 | 6 | 3 |
| 20 | 2 7 30 M. H. W. | 2 3 | 2 5 | 4 | 2 |
| | 2 33 0 E. H. W. | 2 27 | 2 28 | 6 | 5 |
| | 8 42 0 E. L. W. | 8 39 | 8 40 | 3 | 2 |
| 21 | 2 54 0 M. H. W. | 2 51 | 2 53 | 3 | 1 |
| | 9 4 30 M. L. W. | 9 2 | 9 5 | 2 | 0 |
| | 3 14 50 E. H. W. | 3 13 | 3 18 | 2 | 3 |
| | 9 23 0 E. L. W. | 9 23 | 9 24 | 0 | 1 |

## *A* TABLE *of the tides observed at* Bayonn[e]

| 1680. | Time of the high and low water observed. | Time calculated. By the first table. | By the second table. | Diff. by the first table. | D... |
|---|---|---|---|---|---|
| September. | | | | | |
| Days. | H. M. S. | H. M. | H. M. | M. | |
| Full moon at 7 hours, 30 minutes, E. 22 | 9 39 0 M. L. W.<br>9 56 30 E. L. W. | 9 10<br>9 35 | 9 10<br>9 35 | 29<br>21 | |
| 23 | 10 11 30 M. L. W.<br>10 25 0 E. L. W. | 10 1<br>10 22 | 9 58<br>10 19 | 11<br>3 | |
| 24 | 10 47 0 M. L. W.<br>11 2 30 E. L. W. | 10 43<br>11 1 | 10 38<br>10 56 | 4<br>1 | |
| 25 | 11 19 30 M. L. W.<br>11 32 0 E. L. W. | 11 19<br>11 37 | 11 12<br>11 28 | 0<br>5 | |
| 26 | 11 50 0 M. L. W. | 11 55 | 11 45 | 5 | |
| 27 | 0 7 0 M. L. W.<br>0 23 30 E. L. W. | 0 13<br>0 33 | 0 2<br>0 22 | 6<br>9 | |
| 28 | 0 35 30 M. L. W.<br>0 59 30 E. L. W. | 0 53<br>1 13 | 0 42<br>1 3 | 18<br>13 | |
| 1 ft quarter, the 30th of Sept. at 11 h. 4 min. E. 29 | 1 14 30 M. L. W. | 1 31 | 1 26 | 17 | |
| October. 1 | 3 19 0 M. L. W.<br>9 30 0 M. H. W.<br>10 3 30 E. H. W. | 2 55<br>9 11<br>9 47 | 2 55<br>9 8<br>9 41 | 24<br>19<br>16 | |
| 2 | 10 52 30 M. H. W.<br>11 22 0 E. H. W. | 10 28<br>11 11 | 10 21<br>11 5 | 24<br>11 | |
| 3 | 0 5 0 E. H. W. | 11 50 | 11 50 | 15 | |
| 4 | 0 27 0 M. H. W.<br>1 35 30 E. H. W. | 0 22<br>0 54 | 0 24<br>0 51 | 5<br>41 | |

XIII. *An examination of the silk of spiders,
by M. de* Reaumur *; translated by Mr.*
Chambers.

The publick diflike had long lain heavy on
the fpider, and notwithftanding fo many curious
things publifhed of it by feveral learned men, it
was ftill looked upon with fome degree of hor-
ror, and held a dangerous, or at leaft a ufelefs.
infect, till a year ago, that M. *Bon*, firft prefi-
dent of the chamber of accounts at *Montpellier*,
procured it a more favourable regard. There is
room to hope, that the extraordinary things he
has fhewn of it, may be turned to fome advan-
tage ; fince, like the filk-worm, it fpins a filk,
capable of being made into the fineft works ; of
which the gloves and ftockings he then prefented
to the royal academy of *Montpellier*, are an in-
conteftible proof. A like prefent of gloves has
been fince made to the royal academy of *Paris* ;
where the difcovery, having fome air of ufeful-
nefs, was thought worth the purfuing ; either that
the publick might reap the fruit thereof, or at
leaft that the infamy might be removed of ne-
glecting any thing that might be beneficial. The
fate of the filk of worms is notorious, which tho'
known had remained almoft ufelefs for many
ages ; and it had been inexcufable to let the filk
of fpiders run the fame fortune.

The academy therefore thought fit to depute
two members, to purfue what M. *Bon* had fo in-
genioufly hinted ; and I was one of thofe the
choice fell upon. I accepted it accordingly, not
only as the publick good feemed concerned therein,
but our illuftrious prefident, the *Abbé Bignon*,

* Nov. 12, 1710.

feemed kindly to intereft himfelf in behalf of the poor fpiders.

To go orderly to work, I held it my chief bufinefs to confider the filk of fpiders, with regard to that of worms, in order to find by this comparifon, whether the new filk would be of any fervice, anfwerable to what we receive from the old ; for it was no longer any queftion, whether the fpiders fpin at certain times a fort of filk fit for manufacturing ; this had already been demonftrated by M. *Bon*, paft all exception ; but whether they fpin a filk that may be of advantage to the publick ? To determine this, 'tis not enough to find the fecret of feeding and bringing up fpiders, as fome learned men have fuppofed, but to find whether, granting this fecret known, the fpider filk will come as cheap as the other ; or, in cafe it be dearer, whether this inconvenience be compenfated by any other advantage : Thefe two points were what I chiefly aimed at in my inquiry, and to thefe all I fhall offer in the enfuing difcourfe is reducible.

The artifice, which fpiders make ufe of to catch flies, has taught all the world that they feed on thofe infects ; but 'tis obvious, that there is no feeding a number of fpiders, fufficient to furnifh a filk manufacture with flies. What ftratagem muft be ufed to take the daily quantity of flies neceffary for fuch purpofe? and tho' the method of catching flies was never fo much improved, what better fhould we be, when 'tis vifible, that all the flies in the kingdom would hardly fupply fpiders enough to make even an inconfiderable quantity of filk, as may be eafily inferred from what we have to urge upon our fecond article.

Recourfe therefore muft be had to fome new food, whereof a fufficient quantity may commodioufly

dioufly be had.   Now the rapacious difpofition of the fpider is a fufficient indication, that this is not to be had from plants, and that neither their flowers, leaves, nor fruits can afford them a proper nutriment.   I did not omit, however, to try fuch foods, leaft I fhould have it to reproach myfelf with neglecting any thing, and becaufe I was aware, that in matters of experiment, things frequently happen otherwife than was expected; but nothing I could give them of this kind proved any food at all.

Yet 'tis vifible, that flies are not the only food as may fupply them; for tho' the houfe and garden fpiders live chiefly thereof, yet I have frequently obferved them fall with equal appetite upon other infects, which happen to be caught in their toils; and the fpiders in particular, which inhabit the holes of old walls, had made me further fenfible, that no infect comes amifs to them; for, upon vifiting their cells, I have ufually found the carcaffes of diverfe kinds of infects therein, as millepedes, caterpillars, butterflies, &c.

My bufinefs therefore feemed to be to find fome infect, whereof there is ftore enough to be eafily had, and nothing but earth-worms appeared to anfwer this view; as to quantity, the gardens and fields are full of them; witnefs the allies of gardens, which, after a rainy night, are covered over with little round bits of earth, each-whereof covers a hole, at which an earth worm had arofe, nor is there any difficulty in taking them, provided you go in the night with a candle, and remember never to feek for them after a fit of dry weather.

'Tis true, I have never found any earth-worms in fpiders webs, or holes, but this was no great objection, confidering the weight of the infect,

and its keeping altogether on the ground, which makes it impoffible for the worm ever to come within the fpider's clutches. In effect, my expectation was anfwered ; for fhutting up feveral large fpiders of diverfe kinds, which had overlived the winter, and given them pieces of worms to feed on, they were preferved alive thereby.

But it was by no means enough to convince me, that this food was proper for fpiders, to find them live feveral months without any other ; an experiment, I had formerly made, rendered this very precarious, having kept a houfe fpider alive above 3 months, without any food at all. 'Tis known befides, that the young fpiders, hatched in *September*, lived 8 or 9 months without eating.

But as I had inclofed my fpiders in boxes, covered with glafs, I could eafily obferve whether they meddled with the food that was given them, and accordingly have frequently feen them attack pieces of worms, which would ftir, notwithftanding their feparation from the reft of the body, after the like manner as we find them attack infects, which have fome ftrength ftill left, after being taken prifoners in their nets, the various motions of the pieces of worms having ferved to ftir up thefe infects of prey. It muft be added, that they maintained their bulk and vivacity, which thofe I had left without food did not ; and what is ftill more decifive, feveral of them made balls or cods, and laid eggs therein.

I afterwards tried other kinds of foods, to fee whether they might not be equally proper for them ; for how commodious foever worms might be, flefh would have been ftill more fo : but I found, that they did not affect it, and that if it fell in their way, they rarely faftened on it. The

reafon may be, that the fierce and rapacious difpofition of the fpider, muft be urged and excited by living animals.

Yet I bethought myfelf of another food, which might make up this defect, by the exquifitenefs of its tafte, the young fpiders, which have juft left their cods, preferring it to all others ; but what determined me to ufe it, was the refemblance it bore to the tender flefh of infects, which the fpiders delight in, I mean the fubftance, which fills the feathers of young birds, before they be arrived at their full growth. 'Tis known, that upon plucking fuch feathers, they are bloody at the end, and that their quill or barrel is foft ; fo that either fqueezing, or cutting it up, we find it repleat with a foft fubftance, interfperfed with numerous veffels, which difcharge blood upon cutting them ; having plucked fuch feathers from young pidgeons, and even old ones, which had fometime ago been ftripped of the large feathers of their tail and wings, I divided them into little pieces, a line, or $\frac{1}{2}$ a line long, and thus threw them to the fpiders, who feemed well pleafed therewith, and efpecially the young ones ; fo that I could fee 5 or 6 clufters of them about a fingle piece, each of them fucking at the fide it was cut on.

Thus far things fucceeded to my wifh, proper foods being found out, which alone feemed wanting ; and 'tis likely other equally commodious ones might be difcovered, even among the infect-kind, tho' what we have already propofed is eafier to come at than the leaves of mulberries, wherewith filk worms are fed. It may be had without any trouble in all countries, and without running any danger from fharp winters. The poulterers will furnifh young feathers enough, or

they

they may be had by breeding up chickens, or pidgeons, and pulling off their feathers from time to time, which will not hinder their laying eggs, and bringing young ones, as I have found by experience ; but it will appear from what follows, that a large deduction is to be made, when we come to raise spiders enough to furnish a silk manufacture.

As soon as the young spiders quit the silken ball they were inclosed in, they appear perfectly at peace, and work in concert upon the same web, some of them spreading new threads over those already drawn by others; but such union does not hold long. I had distributed 4 or 5 thousand spiders, just fresh from their balls, into several boxes, putting 2 or 3 hundred in some, and one hundred, or fifty, even less in others. The boxes were about the length and breadth of a card, and were as high, and as wide, which was room enough for such little animals. Finding them gather upon the glass, which covered the boxes, I made an aperture in each about a line below the glass, and thrust a card through the same, which rested on the width of the box, and stopped the aperture close enough to prevent the spiders escaping. On this card, I laid such food as I had found suitable for them, disposing it so near the glass, or upper surface of the box, that the spiders might have their food at hand ; and to make those at the bottom, or on the sides of the box, go in quest of it, I had taken care to punch store of holes in the card ; by this means a great number of spiders might be fed in a very little time ; for the first days one might see them fall on their food with eagerness, several of them fastening upon the same bit of feather.

But

But their natural fiercenefs foon declared itfelf; the biggeft and ftrongeft took a liking to eat the fmaller and weaker fort. In effect, every time I viewed them, I could fee fome little one, which had fallen a prey to another fomewhat bigger; fo that in a fhort time there was fcarce one or two left in a box.

I knew that the large fpiders fometimes fight together, when they chance to meet, but was of opinion that they would grow more fociable by being bred together, as we find the chickens and turkies, raifed in the fame yard, live amicably enough, tho' they always make war upon new comers. But the truth is, thefe young fpiders devour each other much more than the big ones: whether it be that thefe laft ftand lefs in need of food, or that being heavier, and more bulky, they do not care to ftir.

This inclination of the fpiders to eat one another, is, in fome meafure, the caufe that their number is fo fmall, confidering the prodigious quantity of eggs they lay. 'Tis true, there are feveral forts of infects that prey upon them. *Pliny* mentions fome fpecies of hornets and lizards, which make them their ordinary food. I have feen the little brown wall lizard catch them, with infinite addrefs; but notwithftanding all this, we fhould find them incomparably more numerous were it not for their eating one another.

No other way therefore feems left for bringing up fpiders, but to lodge them feparately. One may provide, for inftance, boxes, which are fubdivided into feveral little apartments, forming fo many diftinct cells, which I have practifed accordingly; but to give each fpider its food apart, would occafion an expence very difproportionate to the profit accruing from them. This,

however,

however, might be bore withal, were not the silk of worms to be had on better terms.

I am senfible, that ways might be found of abridging this trouble of giving them food, and even have contrived some myself, which I do not think neceffary to defcribe here; but do what you will, 'tis ftill to be feared, that much more time will be employed therein than is neceffary to give silk-worms their food.

The diftributing the fpiders into feparate cells, does likewife draw on a new difficulty, which greatly leffens the advantage they have over silk-worms in point of fecundity; for the benefit of this cannot be had without keeping a large number of eggs, which have been fecundified by copulation, nor is this practicable without putting fpiders together. I am senfible there is a time when a milder fermentation of juices divefts thefe animals of their natural fiercenefs, and that they may then be put together without danger; but how fhall we know this precife time, which withal is to be a little before that when they are difpofed to lay eggs? It would be eafy finding, whether they lay all their eggs nearly on the fame days of the year; but there are feveral months difference between the times when fome lay from that of others.

The fecundity of fpiders is prodigious, as is fully fet forth by M. *Bon*; but, after all, the like may be faid of silk-worms, even tho' we fhould only fuppofe them to lay about a hundred eggs, whereof hardly 40 afford worms that make their balls; whereas fpiders lay 6 or 7 hundred, and yet all the silk-worms I have brought up on this occafion, yielded at leaft 3 or 4 hundred eggs a-piece. 'Tis obvious therefore that the number of silk-worms might be multiplied, as much as we

pleafed,

pleafed, did that only depend on the quantity of their eggs, as abundantly appears from the large ftock of filk they now produce in *Europe*, where anciently there were no worms at all; it would be eafy therefore in courfe of time to have a quantity of filk-worms, as much furpaffing thofe we now have, as thefe furpafs that little number firft brought from the eaft; but what hinders us from increafing our ftock is the trouble of lodging, feeding, and attending them; for if the quantity of filk was increafed, its price would be diminifhed, and, in that cafe, would not pay the charges of raifing the worms. Silk-worms therefore feem clearly to have the advantage over fpiders, as to the facility of raifing them; fo that we are to have no great expectations from the new filk, unlefs it have fome other advantage over the old, either in refpect of beauty, ftrength, or the quantity it yields, which makes our fecond article of inquiry.

As all the forts of fpiders do not afford a filk capable of being manufactured, and as thofe which do furnifh fuch filk, only fpin it to form balls, or cods, wherein to lay the eggs; for as to the webs they make to catch infects, they are ufually too fine for any purpofes of ours, it feems necef-fary to give a general idea of feveral fpecies of fpiders, to which all the reft are reducible, and of the different manner wherein their feveral balls are formed, in order to fhew which of them afford filk to the beft advantage in thefe countries.

M. *Bon*, who has likewife confidered fpiders, with regard to their filk, reduces them to two principal kinds, *viz.* the long-legged fpiders and the fhort-legged ones, which laft, he informs us, alone furnifhed the new filk; but this divifion, which would have great advantages, on account

of its fimplicity, fcarce feems fufficient to diftin-
guifh the filk fpiders from others : for how fhall
we know precifely which to call fhort-legged fpi-
ders, and which long-legged ones ? There are
fpiders, whofe legs are of a middle fize, between
the longeft and fhorteft, and on the which of the
two kinds are thefe to be ranged ? On the foot-
ing of the former divifion, it wou'd be difficult
to decide whether they yield a filk or not; but
this is not the worft, for the divifion would be
apt to lead people into ufelefs pains, the generality
of fpiders, which, according to this, fhould pro-
mife moft filk, yielding none at all. Such are
the feveral kinds of wandering fpiders, and the large
brown fpiders inhabiting the holes of old walls,
whofe legs are fhorter than moft of thofe which
yield filk, though they yield none themfelves.

I rather choofe to range all fpiders under two
claffes ; the firft including the feveral fpecies,
comprized by M. *Homberg*, under the general
name of wandering fpiders, being fuch as do not
lay nets for catching of infects, but roam abroad
in queft of them. Thefe fpiders fpin but little,
and this only to make a cod, or cover, for their
eggs, which fome of them make hemifpherical,
and leave it hung upon ftones, or hid under
ground, or even upon trees, or among herbs ;
others give it the figure of a ball, and are
too fond ever to ftir from it, but carry it
about with them, always hanging to the *papillæ*
about their *anus*, in fuch manner that the ball
only feems to make one body with the fpider ;
which, upon this occafion, appears a little big-
ger than it fhould do. Catching one of them, and
taking its ball away, you will fee the fpider re-
fume it, as foon as fet at liberty, with great eager-
nefs, taking it in its feet, and clapping it firft

under

under its belly, and here letting us into the artifice it ufes in its ordinary carriage; for bending its hind part till it reach the little ball, and then rubbing the fame ball very fwiftly with the *papillæ*, a vifcid liquor is preft out of the fame, and fmeared over part of the ball, which by this means is ftuck faft to the *papillæ*. The parts thus rubbed are eafily diftinguifhed, as being whiter, and clofer than the reft; nor does the animal's tendernefs end here, but when its young ones are hatched, it bears them on its back, where the young fpiders likewife difcover a notable dexterity in their arrangement about their mother's body, being fuch, that there is no perceiving them when fhe walks, only her body, upon this occafion, appears fomewhat rougher than ufual.

The texture of the balls of this kind of fpiders, is very clofe, and ufually white or greyifh; but, befide that it only yields very little filk, what it does yield, is not fit for manufacturing.

The fecond clafs includes all the fpiders, which lay nets for catching infects. Thefe I divide into four principal kinds, each whereof might be fubdivided into feveral fpecies, if an exact hiftory of fpiders were intended: the firft kind contains all the fpiders which make webs of a clofe texture, and ftretch them as parallelly to the horizon, as the weight of the web will admit. Such are the houfe fpiders, which lay their webs in the corners of rooms, and fome fpecies of garden fpiders, which make like webs, and placed after the like manner as thofe of houfe fpiders.

Thefe all of them inclofe their eggs, which ftick a little to one another, in a fort of web, not unlike either as to ftrength or colour. The common ones they lay for flies; fo that we have but little to expect from thefe towards a filk manufacture.

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The fecond kind includes the fpiders, which inhabit the holes of old walls. Thefe line the wall all around their hole with a web, and within the hole itfelf, make another web of a tubular figure, by which they enter and come out of their holes; but neither is the ball, wherein thefe fpiders inclofe their eggs, any ftronger than their common web; fo that thefe two are out of the queftion.

In the third kind I rank all the fpiders, whofe nets have no proper texture refembling that of a web, but confift only of a number of threads ftretched every way. This kind might be fubdivided into a great number of fpecies, which form their balls in different manners; fome make them fegments of a fphere, and faften the flat fide to a leaf, and the firmnefs wherewith they brood over them is invincible; for notwithftanding their natural fiercenefs, if the leaf be taken away this ball is faftened on, the mother fpider will go with it, and never leave it, unlefs by force, till the young ones are hatched. Thefe balls are very white, and of a clofe texture; others make two or three little ruddy balls, and lay their eggs therein, hanging them up by threads in fome open place, but taking care to cover them with dry leaves, to prevent paffengers from feeing them; to this purpofe hanging their leaves upon thread at fome diftance from the ball; others make their ball of a pear form, and hang it by a thread, like the pear fufpended by its pedicle.

Thefe balls are all of a clofe texture, but their filk too weak for working; perhaps, thofe in the pear form might be of fome ufe; but they are fo fmall, and confequently contain fo little filk, as fcarce to be worth minding.

Laftly,

Laftly, the fourth kind includes thofe fpiders, which make webs confifting of feveral threads, all placed in the fame plane, and proceeding from the fame point, like fo many *radii* of a circle, which terminate in its circumference. Acrofs all thefe threads, goes another thread, which winding fpirally, is faftened in diverfe places upon each thereof. Thefe webs are ufually perpendicular to the horizon. M. *Homberg* calls this fort the garden fpider; and accordingly we find it very common among wood, bufhes, &c. Under it are included feveral fpecies of fpiders, different in fize, colour and fhape.

Thefe fpiders lay their eggs one upon another, in fuch manner that the mafs they compofe, has the figure of a flatted fphere, or rather an elliptic fpheroid. Some of them glue their eggs to one another, by a vifcous matter wherewith they are moiftened, when they come from the body; but thofe of others are left loofe. The inmoft threads, which inveft thefe eggs, are wound clofer than the reft, which are but loofly twifted, much like the outward threads of the balls of filk worms.

Moft of thefe fpecies of fpiders fpin a filk, which is fit for manufacturing; tho' what fome of them may afford is too weak to fuftain a common loom.

Spider filks may be had of more diverfity of colours than that of worms, which is always white or yellowifh; whereas the fpiders balls, befides white and yellow, afford fky blue, brown, and a fine coffee colour.

The fpiders, which yield the coffee-coloured filk, are very fcarce, at leaft I have met with none, but in a few broom fields, where I likewife found fome of their balls, the filk whereof

is very ftrong and beautiful. They are formed after a different manner from all the fpider balls above-mentioned ; the eggs are inclofed in a brown filk loofly wound over them as in other balls ; but this brown filk is itfelf inclofed in another ball of greyifh filk, whofe texture is very thick and clofe, and much like what remains of a filk worm ball, when part of it has been wound off.

The fpiders lay eggs, and fpin filk to inclofe them in feveral months of the year, not only in *Auguft* and *September*, as M. *Bon* has obferved ; but fome of them likewife in *May*, and others in the following months, thofe which lay fo early are fuch as have furvived the winter ; whereas thofe M. *Bon* fpeaks of, are only hatched in the fpring, and confequently lay their eggs much later than the former.

Spiders, we have already fhewn, fpin two kinds of threads ; one ferving for a web wherewith to catch infects ; and the other only as a cod, or cafe, to keep their eggs in ; but it may be proper here to add, that the two threads only differ from each other by their greater or lefs ftrength: and to explain how the fpider is able to fpin a thread of this or that degree of ftrength at pleafure, we fuppofe it already known, that near the *anus* of the fpider are feveral *papillæ*, which do the office of a wier-drawer's iron, and mould or fafhioned the liquor, which paffing through them, and afterwards drying, becomes the filk. The fpiders, we have here to do withal, *viz.* thofe which afford a filk proper for manufacturing, have 6 fuch *papillæ*, 4 of them very fenfible, and two fcarce vifible without a magnifier, being placed near the bafes of the two large *papillæ* neareft the *anus*. Each of the

6 vifible ones is itfelf compofed of other lefs and invifible ones, as may eafily be fhewn, by fqueezing the belly of a fpider between two fingers, till the liquor in the *papillæ* begin to iffue forth, and then applying another finger upon one of them, for drawing it gently away again, feveral threads will follow it, all of them vifibly diftinct from each other, from their very rife, and which of confequence mutt have paft thro' different holes. Thefe threads are too fine to be well told ; yet this I can affirm, that I have frequently feen above 8 or 10 fpring from a fingle *papilla*. In effect, more or fewer of fuch threads are drawn from a *papilla*, according as the finger is preffed ftronger, or upon a larger part of its end ; whence it is eafy to conceive how the fpider makes its thread finer or ftronger at difcretion ; fince not only the applying more of the 6 vifible *papillæ* againft a folid body, but even applying them more forcibly, or a larger part of each produces a thread, confifting of a greater number of other threads, and confequently bigger and ftronger.

There mutt be about 18 times as many threads, fuch as they iffue from the holes of the *papillæ*, to make one of the threads of a ball, as a thread of a web. If the quantity of threads, which go to the one and the other, be proportional to the ftrength ; for a weight of 3 grains, hung to one of the threads of a web, ufually breaks it, whereas 40 grains will not break a thread of their ball.

But if thefe threads of the fpider's ball be ftronger than thofe of its web, they are weaker than thofe of the filk-worm's ball, tho' in a lefs proportion. A thread wound from one of thefe laft balls will ufually fuftain 2 drams and ½ ; and confequently a thread of a fpider's ball is, to that of a filk worm's ball, about as 1 to 5, which

<div align="right">feems</div>

feems to be another advantage of the ancient filk above the new.

Indeed each thread of a fpider's ball is flenderer than that of a filk-worm's ball, nearly in the fame proportion as it is weaker; but this hardly makes up for the difadvantage, fince the more threads, the more difficulty there is in joining them; to fay nothing of the danger, that all the threads do not draw alike, and confequently that their affemblage have not the fum of the ftrengths each thread would have a part. This multiplicity of leffer threads, which compofe each thread of a fpider's filk, to make it as thick as that of a filk-worm, may likewife contribute to render fuch works as are made of this filk lefs gloffy than thofe of common filk; for what we call glofs or luftre in a ftuff, being only the effect of its reflecting more coloured light of any kind than another ftuff of the fame colour, the more little interftices there is in a thread of filk, the lefs gloffy it muft appear, fince it will reflect the lefs light. Now 'tis obvious, that the little interftices will be more numerous in a thread, thus compofed of feveral different ones, than in another thread of the fame thicknefs, wherein there is no diftinction; the parts of the vifcid liquor they are formed of, touching each other in more places than feveral diftinct threads can poffibly do. Suppofing therefore each thread of fpider's filk to be of itfelf equally gloffy with that of a filk worm, 'tis certain, that when 5 fuch threads are combined together, to make another of that natural thicknefs, of a filk-worm's thread, this compound thread, and the work formed thereof, will be lefs gloffy than thofe of the common filk-worm.

This

This would be true on a suppofition that each fimple fpider's thread is naturally as gloffy as a fimple filken thread ; but this fuppofition is, perhaps, too favourable to the fpider's filk, for we find, that the more frizled a thread is, the lefs gloffy it is found : thus we fee, that wool, whofe fibres are naturally more curly than thofe of filk, is lefs gloffy withal. Now each hair or thread of fpider's filk is certainly more frizled, than a hair of the worm's filk, and confequently muft have lefs luftre. The reafon of this diverfity is evidently owing to the manner of winding, or reeling the two; for in winding a thread loofely, the fprings of all its little parts are left at liberty to act with all their ftrength ; and thus to bend or curl it this way and that according to their feveral directions ; whereas, in winding a thread after a clofer manner, the action of the fpring of the component parts is prevented, and even weakened by the violence of the new fituation. This will be more readily allowed if we can confider, that the firft threads of a filk-worm's ball, which themfelves are twifted in, in a loofer manner round the reft, are much lefs beautiful and gloffy than thofe which form the body of the ball, which are wound very clofely.

This fame loofe manner wherein the fpider's threads are wound, contributes on another account to diminifh the luftre of the filk they furnifh, *viz.* by preventing their being wound, or reeled, as thofe of filk-worms are ; and obliging the balls to be carded before they be fpun : hence it is obvious, that the large threads of filk when they come from the fpinner, muft confift of an infinite number of fhort hairs, and confequently cannot appear with the beauty, evennefs,

and luftre of another thread of the fame thick-
nefs, compofed only of hairs, equal in length to
itfelf, by reafon all the ends of fuch fhort hairs
muft needs produce little inequalities in the com-
pafs of the thread, which will weaken its luftre.
If any proof of this were needed, it were eafy to
obferve, that the filk even of the filk-worm balls,
when carded, is much inferior to that reeled di-
rectly from the pods.

Suppofing that only two of the *papillæ* had
furnifhed threads to make a fpider's web, and that
each of thofe *papillæ*, which ufually furnifh
threads compounded of feveral others, had only
furnifhed a fimple one, the threads of the web
being 18 times weaker than thofe of the ball; this
laft thread, which we have already obferved is 5
times fmaller than that of a filk-worm, muft con-
fift of 36 hairs or threads at leaft; this reflection
may, perhaps, affift the imagination to conceive
the prodigious divifibility of matter; for, how
minute muft a thread be vifible, notwithftanding,
to the eye, which is no bigger than the hundred
and eightieth part of a fingle hair of filk, which
hair itfelf is only a two hundredth part of a thread
of the fineft fewing filk? For I have frequently
divided fuch threads into two hundred leffer ones,
fo that a thread of fpider's filk of the thicknefs of
common fewing filk, muft really confift of 36000
threads, and may be actually divided into 1000.

A thread of fpider's filk, compofed of thefe
36000 fimple threads, may, perhaps, be fome-
what bigger than a thread of common filk, com-
pofed of 200 fimple threads, tho' the fum of the
bulks of the 36000 threads, and the 200 be the
fame, by reafon it would be difficult, or perhaps
impoffible, to arrange fuch a number of threads
together, without leaving feveral vacuities be-

tween

tween them, by which the bulk muft be increafed; it is for this reafon that the fpider's filk has been thought to yield more in the working, than the common fort; but if it had been confidered, that in return for this, it muft be weaker in the fame proportion, inftead of confidering it as an advantage, people would rather have looked on it as a defect, fince a large body of this filk will only have the ftrength of a lefs body of the other.

We are now come to the laft point, *viz.* to confider what relation the quantity of filk each fpider will furnifh yearly, bears to that procured from a filk worm. By carefully weighing feveral filk worm balls, I find, that the larger kind weigh 4 grains, and the leffer 3, fo that fuppofing 16 ounces to the pound, at leaft 2304 worms will be required to produce a pound of filk. A perfon cloathed in filk little imagines, that feveral thoufand worms have wrought all their life-time to furnifh the matter thereof.

I have weighed with equal care, a great number of fpider balls, and have always found, that about 4 of the larger fort go to balance one filkworm ball, and that they weigh about a grain a-piece; fo that 4 of the largeft fpiders would be required to yield as much filk as a fingle worm affords, fuppofing there were no more wafte in one kind of filk than the other; but in reality the fpider balls are liable to a confiderable diminution, from which thofe of the filk-worms are exempted. This diminution arifes hence, that the fpider balls when weighed, are full of the fhells of eggs, from which the young ones have been hatched, together with other kinds of filth; fo that calculating the diminution of thefe balls, we muft deduct above two thirds from their weight,

fince

fince from 13 ounces of foul fpider filk, M. *Bon* only procured 4 ounces of pure filk ; whereas filk-worm balls undergo no diminution, or at leaft fo little, that it is compenfated by only putting that of fpider filk at two thirds of the whole.

It has been already noted, that a fpider ball before it be cleanfed, is to the weight of a filk-worm ball, as 1 to 4 ; and thererefore when cleanfed, its weight will be to that of the other, as 1 to 12 ; 12 therefore of the largeft fpiders will only furnifh as much filk as one worm ; but each worm makes it ball, as being neceffary for the metamorphofis it has to undergo ; whereas it is otherwife with fpiders ; for their balls being only made to lay their eggs in. If with the whole current of naturalifts before M. *Bon*, we fuppofe their *fpecies* to confift of males and females, and not as he does, take them for hermophradites, only the female fpiders muft make balls ; whence it follows, that fuppofing the number of males and females nearly equal, 24 of the largeft fpiders will only afford as much filk as one worm.

No lefs than 55296 of the largeft fpiders therefore will be neceffary to make a pound of filk, which fpiders muft have been fed feparately for feveral months. In this view, their filk which feems to coft us much more than it is worth, being at leaft 24 times as expenfive as that procured from worms ; and this fuppofing it were not neceffary to keep the fpiders a-part ; but that each fpider would only take up the room of a worm, which however is a falfe fuppofition, for there muft room enough be allowed each to make its web ; if one were to purfue the calculus, therefore, of what this filk would coft, fuppofing the animals fed feparately, and a competent apartment

al-

allowed each, it would evidently appear, that the fpider's filk, would coft incomparably more than that of the filk-worm.

Let it be added, that what has been hitherto faid, only holds of the largeft fpiders; for if the eftimate were made in the common garden fpiders of this country, twelve of them would hardly be found to yield as much filk as one of the balls above-mentioned; 280 of them would only afford the quantity of filk contained in one ball of a filk worm, and confequently 663,552 fpiders hardly one pound of filk.

'Twill be thought pity, that fuch little hope fhould be left of benefitting by fo ingenious a difcovery; but there is fome profpect of refource; fpiders may poffibly be found, which fhall afford more filk, than thofe commonly feen in this country. 'Tis confirmed to us, by the accounts of all travellers, that thofe of *America* are much larger than ours; whence it fhould likewife follow, that their webs and balls are proportionably larger. The filk-worms, which the natives of far diftant countries have multiplied fo much in *Europe*, may give us hopes, that the *American* fpiders may likewife be propagated here, at leaft 'tis worth making the experiment, which alone fhould decide in a thing of this nature; for my own part, nothing that any where relates to the prefent fubject fhall be omitted by me; tho' if any thing ufeful be ftruck out, the chief glory will be M. *Bon*'s due.

XIV.

XIV. *Experiments on the effect of the wind with regard to the thermometer, by M. Caffini the fon* *.

Among feveral phyfical obfervations, that M. *l'Abbé Teinturier*, the arch-deacon of *Verdun*, has fent me fince his return from *Paris* ; he has obferved that when we raife a wind with bellows againft a thermometer, the liquor that is inclofed there in increafes its height; which feems contrary to the impreffion, that the wind makes upon us, which is a fenfation of cold.

To examine, if it had the fame effect upon our thermometers, I applied a pair of common bellows to a thermometer fhut up in a chamber, which in the caves of the obfervatory, ftands at the height of 50 degrees, and was then at 52 degrees ; that is, two degrees above temperate ; and after having blowed for 7 or 8 minutes againft the ball, the thermometer rofe 1 degree.

I repeated the fame experiment fome days after, the thermometer was at 46 degrees, and it rofe alfo a degree in the fame time.

I made ufe of one of M. *Amontons* thermometers, which I had applied to the fire-place of a forge, where there had not been any fire for many years. This thermometer rofe almoft a line in the fpace of 6 minutes that I blowed againft it.

I at laft put the fame thermometer to the fire-place of the forge, where I left it for three hours or thereabouts. I afterwards took it away to fee at what height it was, which I marked 53 inches, 2 lines $\frac{2}{3}$. I blowed againft it for 5 minutes, and taking it away, I found it at the height of 53 in-

* Dec. 13, 1710.

ches,

ches, 4 lines ¦ ; that is, 1 line ¾ higher. I put
it in its place again directly, and after having
blowed for 5 minutes, I found it at 53 inches,
5 lines ¼, Having in the laſt place blowed for
5 minutes more, it roſe to the height of 53 in-
ches, 5 lines ⅚ ; ſo that in the ſpace of ¼ of an
hour, the thermometer roſe above 3 lines.

We may offer for a reaſon of this experiment,
that all motion produces heat, and that thus the
air being excited with violence, acquires ſome
degree of heat; ſo in effect, it ſeems to cauſe in
us a ſenſation of cold, becauſe the particles of air
being driven with violence, apply themſelves
with more force, and in a greater quantity againſt
our bodies, which are warmer than the air that
we breathe.

## XV. *Experiments on the thermometer, by M.* de la Hire *the ſon\**.

My father had formerly obſerved, that having
covered with ſnow the ball of a ſpirit of wine
thermometer expoſed to the air, but not to the
wind, the ſpirit of wine did not alter its height
in the tube, and that afterwards having blown
with a pair of bellows ſtrongly againſt this ſnow,
the ſpirit of wine had remained at the ſame height;
from which it ſeems, that we might conclude that
the temperature of the air, which acts upon the
ſpirit of wine, could not cauſe any alteration there-
in, being ſtrongly driven againſt it; neverthe-
leſs it has appeared quite the contrary, by an ex-
periment related to the academy, by M. *Caſſini*
the ſon. It is to endeavour to diſcover the reaſon
of this contrary effect, that we have again made
the experiment that he has related, but in diffe-

* Dec. 17, 1710.

rent circumſtances, and upon 4 thermometers;
3 of which were ſpirit of wine, and the 4th an
air, one of M. *Amonton* 's.

*Nov.* 27, 1710, about 11 in the morning, we
blowed with a pair of bellows ſtrongly againſt
the ball of a ſpirit of wine thermometer, expoſed
for a great many years in the eaſt tower of the
obſervatory, which is uncovered in ſuch a man-
ner, that it is ſheltered from the wind, and the
ſpirit of wine, that was at 35 parts in the tube,
which marks an air a little warmer than the be-
ginning of froſt, having obſerved, that when it
is at 32, it begins to freeze in the country, did
did not riſe ſenſibly in the tube; we had taken
the precaution before we uſed the bellows to put
them for 2 hours in the ſame place where the ther-
mometer was, for fear that if the whole bellows
were a little warmer than the air which enters in, it
might grow warm in them, and afterwards meet-
ing the ball of the thermometer might heat it, and
make the liquor riſe; and on the contrary, if the
bellows had been in a place where the air was colder
than that where the thermometer was, it might
make it fall, as we obſerved in blowing with
the ſame bellows; and pretty ſoon after the above
experiment, againſt the ball of another thermo-
meter, which was in my father's cloſet, where the
air was much warmer than the outward air, where
theſe bellows had been expoſed; for the liquor
preſently ſunk about ¼ a line, and afterwards roſe
again almoſt to the ſame height, altho' we con-
tinued blowing.

We have alſo made another experiment upon
an air thermometer, which was one of thoſe that
M. *Amontons* firſt made for the experiment of the
heat of boiling water. The ball, which is at the
bottom of the little bent tube, is very big, and

has

has in its lower part quickfilver enough to furnifh the dilatation of the air of the ball, which makes it rife into the tube that is open at the top, and is about 4 feet high, fo that the air does not enter at all into the tube.

*Nov.* 27, 1710, at 4 in the afternoon, the thermometer and the bellows having been in the fame place above 5 hours, and having marked exactly the height of the quickfilver in the little tube, we blowed for 3' againft the ball filled with air, which was compreffed by 25 inches of quickfilver, and we did not obferve any alteration of height in the quickfilver that was in the tube.

The next day, at 10 in the morning, we repeated the firft experiment upon the thermometer, which is in the eaft tower, and the fpirit of wine did not rife fenfibly ; there was another fpirit of wine thermometer near that, the ball of which was much fmaller, and the tube very thin, which we took away and put in a clofe place juft by, where there was a pair of double bellows ; and after having left it there 3 or 4 hours, we blowed againft this fecond thermometer for 7', with the double bellows, and the fpirit of wine rofe 3 lines in the tube.

We afterwards took M. *Amonton's* air thermometer, which had been a great while in this place, and blowed againft the ball, with the double bellows for 7' and the quickfilver alfo rofe 3 lines ; indeed there were 3 or 4 of us a little diftant from the thermometer, during the experiment.

We were afraid, that the number of perfons that there were had caufed this effect ; for this reafon we left the thermometers near one another for 2 or 3 hours, and afterwards with a common pair of bellows, we blowed for 3' againft each of

the

the balls of both thefe thermometers, the fpirit of wine and the quickfilver which were fallen again to the heights that they were before the preceding experiment, both rofe again about a line, but that of the fpirit of wine rather lefs than the other.

We were afraid it was becaufe we had begun with that of fpirit of wine, and that the bellows were heated in our hands ; for this reafon, we left them in the fame pofition, and the bellows near them, and at 6 at night we blowed again for 3′ againft both the balls, beginning with the fpirit of wine thermometer, which rofe a little ; but the air one did not rife at all.

We afterwards blowed with the fame bellows againft the ball of a fpirit of wine thermometer of M. *Amontons*, which is placed in my father's clofet, where the air was warmer than that where the bellows were, and the fpirit of wine rofe in the tube $\frac{2}{3}$ of a line, and did not fall at firft, as it had done the day before.

The 4th, at 7 in the morning, the air thermometer and the great fpirit of wine thermometer, and the bellows having been all night in the eaft tower, we blowed for 4′ againft the ball of the air one, and it did not rife at all ; we then blowed againft the ball of that of fpirit of wine, and it rofe about a line. Afterwards we blowed for above 4′ againft the ball of another fpirit of wine thermometer, which is fmaller, that we had left near the windows of a place juft by, which is clofe and expofed to the fouth, the holes of the bellows being turned againft the windows, the liquor hardly rofe at all ; but by continuing to blow, with the holes of the bellows turned the other way, it rofe more.

At 2 in the afternoon, the fame thermometer having remained in the fame place, and received the

the impreffion of the fun for 3 hours and ½, and the bellows being laid in the place upon a feat about 6 feet from the thermometer, the fun having alfo fhined upon them, we blowed againft the ball of this thermometer, the liquor fell more than 6 lines, the holes of the bellows not being turned againft the windows, but upon turning them againft the windows, and continuing to blow, the fpirit of wine fell again confiderably, tho' the weather was very hot, the fky having been very clear all the day, and the fun fhining there during the experiment.

The 5th, in the morning, we carried the air thermometer and the little fpirit of wine one into the cave of the obfervatory, and after having left them there almoft ¾ of an hour, and the bellows alfo, and having opened and fhut them for fome time, that they might take in the fame warmth as that of the air of the cave, we blowed for 5' againft the ball of the air thermometer, and the quickfilver rofe about 3 lines : but as both the thermometers were a foot diftant from one another, and as before, we blowed againft that of air, we had alfo obferved the height of that of fpirit of wine, we perceived that the fpirit of wine thermometer was alfo rifen 1 line, altho' it had not been at all blowed againft ; we afterwards blowed for the fame time againft that of fpirit of wine, and it alfo rofe about 3 lines, during which time the air thermometer did not rife at all.

We had taken the precaution to carry them into the cave, fearing leaft the light diffufed in the air in the day, might make fome impreffion upon it, which had fome relation to what happened to the *Bolonian* ftone and other *phofphorus.*

Afterwards we applied a piece of cloth two or three times double againft the ball of the air ther-

mometer

mometer, and blowing violently againſt it, it roſe only one line, and during this time the ſpirit of wine thermometer, which remained at the ſame place, roſe ½ a line ; we afterwards applied the cloth againſt the ſpirit of wine thermometer, and having blowed againſt it for the ſame time, it roſe alſo ½ a line : but the air thermometer did not riſe at all during this time, any more than in the preceding experiment.

Altho' in general, the experiments which we have juſt related, ſeem to deſtroy the old one, that my father had made, they neverthelefs fur-niſh a means of giving the reaſon of explaining the difference found between them.

For the ſnow, which was upon the ball of the thermometer, and thro' which the air being driven by the bellows paſſed, was cold enough to chill the particles of air, a little leſs cold than the ſnow, which were applied in a large quantity, and in a little time, by the means of the bellows againſt the ball of the thermometer, and had made the liquor riſe. One can hardly doubt, but that this is the true reaſon of this laſt experiment, and it ſeems, by the means of it, we may give the rea-ſon for all the differences that we have obſerved in thoſe that we have made. But before we abſo-lutely decide, we believe it is neceſſary to wait, till we have made the two following experiments : the firſt would be to blow againſt the ball of a thermometer, during a very great cold ; and the ſecond, during a very great heat, that we might ſee, if what ſhould happen in the extreme, was conformable to that which happened in and about the mean ſtate.

The 16th, at 8 in the morning, a ſpirit of wine thermometer, and ſome water in a veſſel, being put all night in the ſame place, we put this

ther-

thermometer into the water, and after having left them there a good while, we did not at all observe that the spirit of wine had changed its height in the tube ; we afterwards drew the thermometer out of the water, we wetted a cloth in this water, we applied it two or three times double upon the ball of this thermometer, and we blowed strongly with a common pair of bellows against the linnen for 4 or 5', without the spirit of wine changing its height.

Having left the thermometer in this state for an hour, we had a mind to repeat the experiment. We took away the linnen from the ball of the thermometer to make the spirit of wine take the same degree of heat, as the air of the place where it was; and in waiting till it had taken it, we had a mind to fee, if, by agitating it in the air, there would not happen the same thing to it as by blowing upon it, which succeeded; for having shaken it strongly in the air for 8', the spirit of wine rose 2 lines in the tube; afterwards having let it rest some time, it did not change the height at all. We afterwards put it for 8' into the same water where it had been at first, and the liquor fell about a line, but this was only during the 4 last minutes ; we drew it out of the water and having applied the wet cloth to it, we blowed strongly for 8' against the cloth, and the spirit of wine rose again to the same height that it was at before it had been plunged in the water.

The 17th, at 9 in the morning, the same spirit of wine thermometer having remained all night in the east tower of the observatory, and several pieces of marble which we had put there, we applied them to the ball of this thermometer, and in half an hour the spirit of wine funk in the

tube

tube above a line, and afterwards continuing to examine it, we perceived that it was a little rifen. During this experiment, the great fpirit of wine thermometer, which is always in this tower, was rifen about 2 lines ½. This experiment would feem to prove, that the marble cooled more than the fpirit of wine.

XVI. *Obfervations on the little eggs of hens without yolk, vulgarly called cock's-eggs, by* M. Lapeyronie, *of the academy of* Montpelier ; *tranflated by Mr.* Chambers.

The prejudices of birth and education frequently feed people up in errors, even upon matters of fact, which it is no lefs incumbent on academies to difabufe them of, than to tell them new truths.

'Tis a common opinion, even among perfons of fenfe, that cocks lay eggs, and that thefe eggs being hatched in a dunghill, produce ferpents with wings, called bafilifks *; and what is more, they hold, that the very looks of thefe bazilifks ftrike people dead ; yet the whole fable has

no

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* *Sunt etiam quædam ova majora, alia minora, alia etiam minima quæ vulgo in* Italia Centinina *dicuntur & mulieres noftræ hodie (ut olim) a Gallo edita & bafilifcus productura fabulantur. Vulgus (inquit Fabricius) putat exiguum hoc ovum effe ultimum Gallinarum, cum jam centum ova gallina pepererit (unde* Centininum *vocant) quod fine vitello eft : habet tamen cætera, ut chalazas, albumen, membranas, & corticem, verifimile enim eft tum generari, cum vitelli omnes jam in ova migrarunt, neque amplius in vitellario aliquis fupereft vitellus, qui in ovum evadere poffit : ex altera tamen parte; albuminis adhuc modicum fupereft : ex hoc enim modico credibile eft ovulum propofitum creari.* Harvæus in tractatu generationis animalium, exercitatione xii. de ovorum differentiis.

no other ground than an ancient tradition, whofe falfity will be demonftrated by the following facts.

A farmer brought me feveral eggs, one of them bigger than a pidgeon's egg †, affuring me they had been lain by a young cock, which was the only one in his yard, where however there were feveral hens; of this, he made fo little queftion, that he pofitively engaged, that in cafe I would hatch any one of thefe eggs, a ferpent fhould arife from it; and to convince me of this, he told me, I need only open one of them, which I fhould find without any yolk, and that in the room of yolk, there would be the vifible figure of a ferpent.

I opened one of thefe eggs in the prefence of M. *Bon*, and feveral other perfons, who were all equally furprized to find it quite void of yolk, and in lieu of yolk, to difcern a body much refembling a little twifted ferpent *; which I eafily unfolded, after firft ftiffning its fubftance in fpirit of wine, when it appeared as in *fig.* 3. After this, I opened fome others, which, in the main, proved all like the former, with this only diffe-rence, that the feeming ferpent was not equally well expreffed in all. Several of thefe eggs I fhewed the academy; in fome of which was feen a yellow round fpot about a line in diameter, and without thicknefs, fituate on the membrane next under the fhell, directly againft the obtufe end of the egg.

The difference between thefe and the common eggs, which have all a yolk, gave me a curio-fity to examine into the matter, being convinced, that if they were lain by a cock, it muft have had a peculiar organ for the purpofe; and befides the tefticles and two yards have been furnifhed with

† Plate I. Fig. 1.     * Fig. 2.

an ovary and a tube, which muſt have rendered it an hermaphrodite. This indeed is no more than ſeveral animals are in their kind ; and we read ſo many accounts of ſuch monſters, that there was no great difficulty in ſuppoſing, that a cock might chance to be ſuch.

Upon this thought, I opened the young cock, which was ſuppoſed to have lain the little eggs, and in the diſſection I made of it, found two large teſticles, from whence aroſe very regular ſeed veſſels, which terminated each in their ſide, with a little yard in the *cloaca*. The cock in fine was found very vigorous, but utterly incapable of laying for want of organs.

Upon this, I procured ſome of the eggs to be brooded on for hatching ; but opening them after a month's incubation, I could find no alteration in them, excepting that the white was more divided, and alſo more fluid than uſual.

The farmer having now no cock was ſurprized to find a continuation of the ſame eggs, and being very ſolicitous to find whence they ſhould come, watched them ſo narrowly, that at length he aſſured me, they were lain by a hen, which he brought me accordingly.

All the time I kept this hen I found it crow very ſtrongly, much like a cock that was hoarſe : and that ſhe voided by the *anus* a thin yellowiſh matter, not unlike the yolk of an egg dilated in water, and ſhe ſtill continued laying little eggs like thoſe I had opened.

Being ſatisfied of theſe facts, nothing remained but to diſcover the cauſe, which accordingly I ſought for in the *viſcera* of the hen, and ſhewed the academy a bladder about the bigneſs of the fiſt full of limpid water repreſented by CCCCC, *fig*. 4. which was faſtened by the upper root of its

neck

neck G, to the ligament EE, whereby the ovary is fixed to the mouth of the oviduct, and by the lower root to the centre G of the mefentery of the oviduct, which choaked as it were the two parts of the oviduct thus grafped by F F.

This fort of dropfy pinched the two parts F F of the oviduct fo clofe, that their cavity, even when moft diftended, was not above five lines in diameter; and confequently a common egg, fuch as they are when they fall into the tube, could not pafs without burfting either the oviduct or itfelf.

The belly was full of a yellow liquor, wherein little concretions were found floating like pieces of hardened yolk, which formed another very fingular fpecies of dropfy.

Now the large bladder full of water was the real caufe of all thefe effects.————When an egg grafped by the mouth of the tube became feparated from the ovary, and entered into the oviduct, it paffed along, tho' with fome difficulty, beyond the firft choak, but could not get beyond the fecond, both as it was clofer than the firft, and by reafon the white of the egg had been enlarged by an acceffion of moifture, furnifhed from the membranes of the tube it had paffed by: thus the egg being imprifoned between the two choaks, irritated the membranes of the tube, which not being able to expel it, redoubled its contractions, and obliged the hen to give itfelf violent throws and ftruggles, which it expreffed by a fort of cry, imitating, as already has been obferved, the crowing of a cock. By thefe ftruggles the bladder of water was compreffed, and thruft clofe againft its hold; the effect of all which forces was, that the egg, whofe membrane was yet very thin, and which have but very little white, and no fhell, burft; upon which the yellow run out fometimes

into the abdomen, and fometimes into the anus, according to the fituation of the chink, or rupture ; and both the one and the other had befallen this hen, as we have already obferved.

The bulk of the egg being thus diminifhed by a lofs of a great part of the yolk, came forth notwithftanding the choak, and was at length delivered.

It is obfervable, that the white furrounding the yolk, filled notwithftanding its being pierced in the part whereat the yolk run out ; and confequently its wanting the tenfion, one would have thought necefiary for its growth ; for notwithftanding all this, the humour furnifhed by the membranes of the * oviduct, continued to fill and fwell its fpungeous parts ; and in proportion as it fwelled, it expreffed the remainder of the fluid matter of the yolk, which could not withftand by reafon of its iffue ; and therefore commonly was evacuated almoft wholly, only leaving fometimes a few traces in one of the corners of the egg, under the form of a yellow fpot. It might fometimes alfo happen, that a little part of the yolk fhould be left in a body, though I have never found it fo in any eggs I have opened.

* Several perfons pretend, that the white of the egg is furnifhed by the yolk. This obfervation demonftrates, not only that the yolk is not the fource of the white (for how could the yolk, which increafes rather than diminifhes in the oviduct, be fufficient to produce the whole fubftance of the white, which is of a much greater dimenfion than the yolk itfeif, if it did not receive it elfewhere ?) but alfo that the liquor which makes it, does not pafs through the yolk, but after having paffed through the exterior membrane of the egg, it enters immediately into the fpongious body, where it ftops ; if this was otherwife, the humour of the white would have flowed together with the yolk, and its fponge would not have increafed.

While

While the yolk was emptying, in itself, by little and little, the *Chalazas* ranged themselves differently, according to the place where the egg had burst ; if the rupture were aside of a *Chalaza*, the cells about the oppofite *chalaza* fwelling, chofe the other which faftened itfelf to the obtufe end of the egg, as meeting there with the leaft refiftance ; accordingly I have frequently found it faftened in this part, and fometimes even together with the yellow fpot.

But when the aperture happened in a part of the yolk equally diftant from the two *chalazas*, they then wrought in concert to expel the yolk ; and after this, reunited in the centres of the egg, by the contraction of the membrane of the yolk, to the ends whereof they are ftrongly faftened, which made the refemblance of a ferpent much more twifted than when there was only one *chalaza*.

After the yolk had been intirely evacuated, and even followed by the moft fluid part of the white, its aperture was foon clofed up, and cicatrized by the vifcidity of the white, as well as by the fatty matter fmeered over the infide of the oviduct, and even by the humid matter of the egg-fhell lodged at the bottom of this duct.

I have collected fome of this humour, and expofing it to a gentle heat, it formed a fubftance perfectly like the fhell.

There is a great probability, that part of the white went away with the yolk, fince there was only about a third fo much white in one of thefe, as in a common egg.

I have fometimes found the *cicatrix* of the aperture of the membrane whereat the yolk had iffued fo clofely faftened to the part of the fhell corresponding to it, that one could not have fepa-

rated

rated it without tearing, which was not the cafe in the reft of the circumference.

If hens fometimes lay eggs without fhells, it is owing either to fome difeafe, which irritating the tube, makes them expel the egg before its time; or too great a fecundity, which will not give them leifure to ripen them all ; fome hens will, in the fame day, lay a perfect orderly egg, and another without a fhell ; add, that the want of a fufficient quantity of this humour which forms the fhells, may in fome hens be a caufe thereof. It is poffible likewife for a hen to lay eggs like thofe here defcribed, when by the violence of its ftruggles to be delivered of it, or by fome other external caufe the yolk of the egg happens to be burft in the oviduct ; but this caufe not being conftant, the fame hens may at other times lay well conditioned eggs.

Such choakings or compreffions which utterly prevent or annihilate the young ones in the oviparous kind, by withdrawing the matter of the nutriment, would only render thofe of the viviparous kind monfters, by reafon thefe do not carry their nutriment with them, but find it in the womb where they are going, provided fuch compreffion do not deftroy any part effential to the life of the animal.———Hence we are not to be furprifed, that thefe latter furnifh much more monfters than the former.

*An explanation of the figures, tranflated by* J. M.

*Plate* I. *fig.* 1. reprefents an egg without yolk, of the natural bignefs, covered with itsfhell, having one angle acute, and the other obtufe.

*Fig.* 2. reprefents the fame egg opened.

A A A the clear white.

BB the thick white, in the centre of which appears the *chalaza*, or figure of the pretended ferpent.

*Fig.* 3. reprefents the fame *chalaza* taken from the centre of the thick white, and viewed through a microfcope.

*Fig.* 4. fhews the infide of the belly of the hen, which laid the eggs reprefented in the preceding figures.

AAAA the ovary.

BBBBBBBB the oviduct.

CCCCC the preternatural bladder filled with clear water, fituated in the middle of the *abdomen*, laid on one fide to difcover its faftenings ; it covers a part of the oviduct, and ftrangles it in the 2 places marked FF.

D the expanfion, or entrance of the oviduct.

EE the ligament, which faftens the fide of the right expanfion of the ovary.

FF the ligament of the neck of the bladder CCCCC, which ftrangles two places of the oviduct.

GG the faftening of the ligament; the upper one to the ligament EE, and the lower one to the centre of the mefentery of the oviduct.

H the *cloaca* in which are feen two apertures; one of which correfponds with the oviduct, and the other with the inteftine.

IIIII feveral yellow concretions, interfperfed in the *abdomen*, refembling parcels of the yolk of a hard egg.

XVII.

XVII. *A comparison of the observations of the* *eclipse of the moon,* Feb. 13, 1710, *made* *in different places, by M.* Maraldi*.

We have compared the observations of this eclipse made at *Montpellier,* by Meff. *Plantade* and *de Clapiez,* with thofe which are made at the fame time at *Verfailles* and at *Paris* ; and this is the refult of them.

| ʰ | ′ | ″ | |
|---|---|---|---|
| 11 | 15 | 50 | All *Grimaldi* out of the fhadow at *Montpellier.* |
| 11 | 9 | 0 | *Grimaldi* intirely emerged at *Ver-failles.* |
| | 6 | 50 | Difference of the meridians between *Verfailles* and *Montpellier* ; but *Verfailles* is more wefterly than *Paris* by 50″ of time ; therefore the difference of the meridians between *Paris* and *Montpellier,* is 6′ 0″. |
| 11 | 23 | 53 | The eclipfe was 8 digits at *Montpellier.* |
| | 19 | 0 | The fame phafe at *Paris.* |
| | 4 | 51 | Difference of the meridians. |
| 11 | 32 | 40 | The eclipfe is 7 digits at *Montpellier.* |
| | 28 | 0 | It is 7 digits at *Paris.* |
| | 4 | 40 | Difference of the meridians. |
| 11 | 29 | 0 | *Copernicus* out of the fhadow at *Montpellier.* |
| 11 | 22 | 0 | The fhadow quits *Copernicus* at *Verfailles.* |
| | 7 | 0 | Difference of the meridians between *Montpellier* and *Verfailles,* and between *Montpellier* and *Paris,* 6 10″. |

* March 12, 1710.

Pl.1.*Vol.IV.*

*Fig.1.*

*Fig.4.*

*Fig.2.*

*Fig.3.*

J. *Mynde* Sc.

Pl.1.*Vol.IV.*
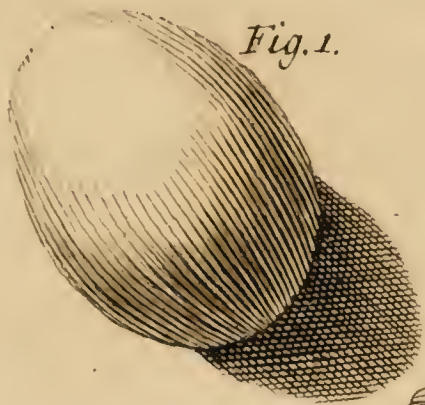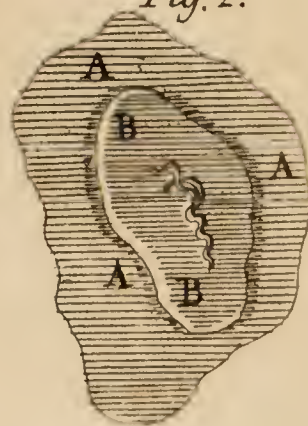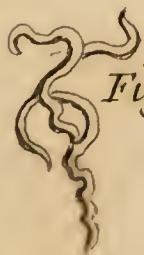
| h | ′ | ″ | |
|---|---|---|---|
| 11 | 42 | 0 | The shadow at the edge of *Menalaus*, at *Montpellier*. |
| 11 | 36 | 15 | The shadow at *Menalaus*. |
| | 5 | 45 | Difference of the meridians between *Montpellier* and *Paris*. |
| 11 | 47 | 2 | *Pliny* at *Montpellier*. |
| 10 | 40 | 0 | *Pliny* begins to emerge at *Verfailles*. |
| | 7 | 2 | Difference of the meridians between *Verfailles* and *Montpellier*, and between *Paris* and *Montpellier* 6′ 12″ by this obfervation. |
| 11 | 47 | 2 | *Pliny* at *Montpellier*. |
| 11 | 41 | 20 | The shadow of *Pliny* obferved at *Paris*. |
| | 5 | 42 | Difference of the meridians. |
| 11 | 53 | 18 | All *Tycho* out of the shadow at *Montpellier*. |
| 11 | 47 | 10 | The shadow at the fecond border of *Tycho* at *Paris*. |
| | 6 | 8 | Difference of the meridians. |
| 12 | 12 | 24 | The eclipfe is one digit at *Montpellier*. |
| 12 | 8 | 0 | The fame phafe at *Paris*. |
| | 4 | 24 | Difference of the meridians. |
| 12 | 18 | 10 | End of the eclipfe at *Montpellier*. |
| 12 | 12 | 20 | End of the eclipfe at *Paris*. |
| | 5 | 50 | Difference of the meridians between *Paris* and *Montpellier*. |
| 12 | 11 | 30 | End of the eclipfe at *Verfailles* |
| | 6 | 40 | Difference of the meridians between *Verfailles* and *Montpellier*. |

By the comparifon of thefe obfervations, the fpots of *Grimaldi*, *Copernicus*, and *Tycho*, give within fome feconds the fame difference of the meridians between *Verfailles*, *Paris*, and *Montpellier*; and that of 6′ 10″ which refults between *Montpellier* and *Paris*, agrees with that which

3                                               has

has been found by the obfervations of the fatellites of *Jupiter*, made in both places, and by the triangles of the meridian. The difference of the meridians which refults between *Paris* and *Verfailles*, by the fame obfervations, is alfo the fame that has been found between thefe 2 cities, by geometrical operations.

This exactnefs comes from the fpots being well determined, and the time of the fhadow's quitting the fecond limb of thefe fpots being well diftinguifhed. There is not the fame exactnefs in the difference of the meridians which refults from the fpots of *Pliny* and *Menelaus*, obferved at *Paris* and *Montpellier* ; becaufe as thefe fpots have fome breadth, they have not agreed perhaps in marking the arrival of the fhadow at the fame term of the fpot, as they have done in the obfervation of the former fpots.

The difference of the meridians which refults from the determination of the digits eclipfed, is a little more than that of the fpots ; which fhews, that we muft not make ufe of thefe forts of obfervations in the finding of longitudes, except when we have no obfervations of the fpots.

Laftly we fee, that in the determination of the end of this eclipfe, all the obfervers agree within about ½ a minute, tho' they are not fo exactly agreed in the determination of the digits eclipfed.

# A TABLE

## OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCXI.

## In the HISTORY.

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I. *Obfervations on the height of the water, which fell at the obfervatory during the year* 1710, *with thofe on the thermometer and barometer, by M.* de la Hire.

II. *A comparifon of our obfervations on the height of the rain-water, and on the barometer, with thofe which M.* Scheuchzer *made at* Zurick, *in* Swizzerland, *during the year* 1710, *by M.* de la Hire.

III. *Experiments to know whether the ftrength of cords exceeds the fum of the forces of the the threads which compofe them, by M.* de Reaumur.

IV. *Remarks on fome colours, by M.* de la Hire.

V. *RefleEtions on fome new obfervations of F.* Feuilleé, *made in the* Weft-Indies, *extraEted from a letter written to M.* le Comte de Pontchartrain, *from* Lima, Dec. 7, 1709, *by M.* Caffini *the fon.*

VI. *An extraEt of feveral obfervations made by F.* Feuilleé *in the* Weft-Indies, *by M.* Caffini *the fon.*

VII. *Experiments on the thermometer, by M.* de la Hire *the fon.*

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IX. *Obfervations of fome eclipfes of the planets and fixed ftars by the moon, made in feveral places, compared together to determine the differences of the meridians, by M.* Caffini *the fon.*

A N

# AN
# ABRIDGMENT

## OF THE

PHILOSOPHICAL DISCOVERIES and OB-
SERVATIONS in the HISTORY of the
ROYAL ACADEMY of SCIENCES at
*Paris*, for the Year 1711.

## I. On *the communication of air in water.*

WE know that the water is quite filled and
impregnated with air. As foon as it is
*in vacuo*, the air which it contained, difengages
itfelf, and goes out, in an infinite number of
bubbles. The mechanifm of the refpiration of
fifhes, confifts only in drawing from the water,
the air that is inclofed in it ; but Mefs. *de la Hire*
had a mind to fee what power obliges it to enter
therein, and whether it enters with a velocity pro-
portioned to the force with which this power im-
pells it.

For this purpofe, they took a crooked glafs
tube, with unequal branches, the longeft of which
being hermetically fealed, was twenty-four inches,
and the fhorteft three. They poured water in it,
inclining it, but did not fill it intirely ; fo that
when they afterwards placed it vertically, there
happened to it the fame thing, as to a tube not
quite filled with quickfilver. There was at the
top of the long branch, fome air a little dilated ;
it occupied four inches, and the water kept up at
fixteen inches, nine lines, above the water of the
little branch : thefe four inches of air, and thefe
fixteen inches, nine lines, of water, made there-

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fore an *equilibrum*, with the intire column of air which preffed upon the little branch; and as they had taken the time when the barometer was in its mean height, this column was equivalent to 27 ¼ inches of quick-filver, or thirty-two feet of water, which are three hundred eighty-four inches : confequently the four inches of air inclofed in the long branch, made an *equilibrium* with three hundred fixty-feven inches, three lines of water, and were more dilated than the outward air in the ratio of three hundred eighty-four, to above three hundred fixty-feven.

The air which touched the water of the little branch, being more condenfed, or which comes to the fame thing, more preffed than that of the long branch, ought therefore to enter into the water, to pafs into the long branch, to rife always through the water, to join itfelf to the air of the top of the tube, to augment its bulk, and its weight, and make the fixteen inches, nine lines, of water, fink. To make the outward air enter into the water in a greater quantity, the little branch opened into a glafs phial, which prefented to the air, a pretty large furface.

This was done the 16th of *March*, 1710; and the crooked tube was left in experiment. Meff. *de la Hire* fully expected, that the water of the long branch would fink, as they had feen it happen to that of a water-barometer which they had; they believed alfo, that befides its defcending in general, by the introduction of a new air into the top of the tube, it would have fome particular variations of height, by the fame caufes with the barometer and thermometer; but the event was abfolutely contrary to all that could be forefeen. At the end of three months, the water was rifen

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about four lines in the tube; and the 26th of *December* it was an entire inch; so that the air which was inclosed in it had lost $\frac{1}{4}$ of its bulk. The variations of the heat, and of the gravity of the atmosphere, had not any effect upon this water.

Mess. *de la Hire* do not yet undertake to explain so unexpected, and so odd a phænomenon. They labour to clear it by other experiments, which perhaps have also their oddnesses, or their wonders.

II. *On the cause of the variation of the barometer*; *translated by Mr.* Chambers.

It is evident from the barometer, that when it rains, or is about to rain, the air usually becomes lighter than before. Now 'tis easy to imagine, that if the air become lighter, it must rain; for that the little insensible molecules of water diffused in vast quantities through all parts of the air, being no longer sufficiently sustained, when the air has lost part of its weight, must of course begin to fall; and by this fall, united together, form drops of rain: thus in the receiver of an air-pump, after about half of the air is exhausted, and consequently the weight of the whole diminished by half, we find a little shower trickle down. —— But then, how comes the air to loose its weight? The answer might be, that where it rains, the winds have driven part of it elsewhere; but M. *Leibnitz*, in a letter to the *Ablé Bignon*, alledges another reason, entirely new.

He asserts, that a foreign body immerged in a fluid, weighs with that fluid, and make part of its whole weight, so long as it is sustained therein; but that if it cease to be sustained, and consequently
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quently begin to fall, its weight will no longer make part of that of the fluid, which therefore becomes lighter of courfe. ———— The application of this, to the molecules of water, is eafy; they increafe the weight of the air while it fuftains them, and diminifh it when it lets them fall; and as it may frequently happen, that the higher mocules of water fhall fall a confiderable time ere they join the lower, the weight of the air will be diminifhed ere it rain, and the barometer will foretel it.

This new principle of M. *Leibnitz* is very furprifing; for a foreign body immerged in a fluid, whether it be fuftained therein, or not, muft ftill weigh, or gravitate. And can its weight bear on any other bottom, than that which fupports the whole fluid? Does this bottom ceafe to bear the foreign body, becaufe it falls? And does not this body, even in its fall, continue to make part of the fluid fo far as it regards its weight? By this account, while a chymical precipitation is proceeding, the whole matter muft weigh lefs than it did, before the precipitation began, which is more than was ever yet obferved.

But notwithftanding all thefe objections, the principle will fubfift when clofely examined. What fupports a heavy body is preffed by it; a table, for inftance, which fupports a lump of iron of a pound weight, will be preffed by it; and will only be preffed, becaufe it fuftains the whole action and effort, which the caufe of weight, whatever it be, exerts on this lump of iron to drive it downwards; if therefore the table yield, and give way to the action of this caufe of weight, it will no longer be preffed by it; and confequently will not bear any thing: after the fame manner, the bottom of a veffel containing

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a fluid, oppofes the whole action of the caufes of gravity on that fluid ; and if a foreign body float therein, the bottom will alfo oppofe the action of the caufe of gravity thereon ; for that fuch body being *in equilibrio* with the fluid, is as to this re-fpect, a real part of it : thus the bottom is preffed, both by the fluid, and the foreign body ; and it bears them both ; but if fuch body fall, it yields to the action of gravity ; and confequently the bottom does not fuftain it, nor will fuftain it till the body falls upon it, during the time of the fall ; therefore the bottom is eafed of the weight of this body, which is no longer bore by any thing, but impelled by the caufe of gravity, which nothing hinders it from yielding to.

To confirm this doctrine, M. *Leinbitz* pro-pofed an experiment.———To the two ends of a thread faften two bodies ; one of them heavier, and the other lighter than water ; but fuch as that, both together, may float on the water : put thefe in a phial full of water, and fufpend this phial in a ballance, where it ftands exactly in equilibrio with another weight ; then cut the thread which ties the two bodies together, and the heavier will begin to fall. In this cafe he holds, that the phial will no longer maintain the equilibrium ; but that the weight, which before was equal to it, will now prevail, and make it mount by rea-fon the bottom of the phial, which bears the whole weight thereof, is now lefs preffed than before. It is evident fuch phial muft be of a fufficient length, that the falling body may not arrive at the bottom, before the phial have had time to mount. In chymical precipitations the veffels are not deep enough, or the matters precipitate too haftily, or fometimes even too flowly, for the effect of this principle to appear, fo that the falling particles

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always, as to fenfe, in equi-librio, with the liquor which contains them.

M. *Ramazzini*, a celebrated profeffor at *Padua*, to whom M. *Leibnitz* had propofed his experiment, made it with fuccefs after a few fruitlefs tryals, and it fucceeded in the fame manner with M. *de Reaumur*, to whom the academy deputed the care of making it, fo that we have here a new view in philofophy, which tho' it arife from a principle well known, is yet very fubtile and abftrufe ; and may give us room to apprehend, that in fubjects which had been the moft canvaffed, many things have ftill efcaped us.

## III. *The defcription of a natural grotto near* Foligno, *in* Italy.

M. *Maraldi* has given the defcription of a natural grotto, which has been found in making the foundation of a houfe, that M. *le Marquis Elifei* built three miles from *Foligno*, in *Italy*. The figure of the grotto is irregular ; its greateft height, which is unequal, is thirty or forty feet, and ten or twelve paces broad. The walls of it are formed by a fine incruftation of marble, of a pale yellow colour ; and they are raifed from fpace to fpace, by columns in *baffo relievo*, of the fame marble. From the top of the vault there defcended other like columns, fome quite to the ground, which were twenty-five feet long ; the others at different diftances ; and the fhorteft were only two or three feet: their diameters were alfo of very different fizes. Among all thefe diverfities, there was a remarkable regularity. The height of the walls, and of the columns, both of thofe that are placed againft the walls, and of

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thofe which defcended from the top, provided they defcend low enough, is divided into two unequal parts, by a cord which runs through the whole; and is in the fame horizontal plane raifed about four feet above the floor. All that is above the cord, is more equal, and uniform, and lefs rugged than that which is below. From the cord, the columns grows thicker to a certain diftance; after which they grow lefs. In this fwelling, M. *Maraldi* found the circumference of the columns to be thirty inches; whereas it is only twenty-two above the cord. This floor of the grotto is unequal, and formed by broad and thin flabs of marble, placed one upon another; and fometimes in fuch a manner, that they make little arches, which, by walking upon them, fink in, and break.

As there is near this place a river, the waters of which have a fulphurous tafte, and fmell, M. *Maraldi* believes, that thefe waters in filtrating thro' the earths, may have brought away fome clay, or fand, which being mixed with fome fulphur, may have formed all the petrifications of the grotto; for he obferves, that the fulphurous waters of *Tivoli* have always a number of little ftones; the affemblage of which forms a fort of *Travertin*; and that probably thefe waters have produced them, fince the common opinion of the workmen is, that this *Travertin* increafes fenfibly. Some finer fands which have been brought at firft, have made the petrefications which are above the cord, more equal, and more perfect; afterwards, fome coarfer fands which have paffed thro' the paths, which the firft had opened, perhaps alfo mixed with too much water, to give it an eafier paffage, have made the lower petrifications, lefs uniform, and not fo well wrought.

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The grotto of *Antiparos*, which was spoken of by the late M. *Tournefort* in the memoirs of 1702*, was also full of pieces of marble ; but they came out of the earth, and raised themselves upwards. And if, as as we have said in the history of 1708, this grotto in the hypothesis of M. *Tournefort* was a garden, of which the pieces of marble were the plants, this of *Foligno* will be also a garden, but inverted since the plants grow from its arch, and descend in this respect like coral.

## IV. *On the reduction of the depth of snow in melting.*

According to M. *de la Hire*'s observations, snow, when melted, is reduced always to the fifth or sixth part of the height that it had. The night between the 13th and 14th of *February*, this year, there fell some snow, which was reduced to $\frac{1}{12}$th of its height ; that is to say, in melting it diminished as much again as usual : the reason of it is, as M. *de la Hire* has observed, that it was very fine, very thin, and all in small flakes, and excessively dry ; which supporting themselves, one upon another, occupied a great deal of space. Because of this dryness, it fastened but little upon the houses; and that which fell upon the north-side, from whence the wind came, was entirely carried away, although there fell six or seven inches of snow.

* Vol. I. p. 419 of this abridgment.

V.

V. *Of the colours of bodies melted by the burning-glass.*

M. *Homberg* has said, that such substances as gold, silver, &c. which being in fusion at the focus of a burning glass, appear to the naked eye, only under the colour of light; and with a prodigious brightness, are seen with their natural colours, if they are viewed through a smoaked glass.

VI. *Of a glew for agat.*

M. *Homberg* has proved, that the glew of cheese, which is proper for glass, is of no use for agat; and that it must have the varnish of china.

VII. *Of a salt found in the waters of* Arcueil.

In *November* 1710, M. *de la Hire* the son, having a mind to make some experiments, had with the water of *Arcueil* filled a bottle, in which there had been wine; but it had been rinsed with two or three waters; he put a piece of lead in it as big as a nut, and afterwards stopped it well with a cork. He left it without touching it, in a place where the sun did not shine, and where there was no fire made. In *January* following, he looked at his bottle, and perceived that upon the convex part of the bottom of the bottle, there was a little white body, as big as a pin's head, and and some days after, not having removed the bottle, he saw that it was a grain of salt of a cubic figure, and that it began to form several others

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round it. They continued forming themfelves till the end of *May*, when there was about twenty of a middling fize, and as many fmall ones. M. *de la Hire* drew fome of them out of the bottle without emptying it, and he found they had the figure of the fea-falt, and a little of its tafte. Having kept them fome days wrapt up in a paper, he faw that they were become white inftead of tranfparent, as they were before, that they were almoft all reduced to powder, and calcined of themfelves ; and that thofe which were not fo, eafily broke, and became a very fine white powder.

As the water of *Arcueil* produces a ftony cruft in the channels where it runs, we might believe, that the matter found in the bottle was of the fame nature, but it had a tafte, and calcined in the air, two qualities which the other has not.

We know that lead diffolves in vinegar, and M. *de la Hire* fufpected, that fome acid particles of the wine which had not been taken away in wafhing the bottle, might have acted upon the little bit of lead that he had put in, and loofened thefe little white grains from it ; but if they had been lead, they would eafily have incorporated with an oil, as that of nuts, which they did not do at all.

There is therefore more probability, that they were falt, altho' it is pretty extraordinary, that they fhould be thus formed in fo pure a water as that of *Arcueil*. In emptying the bottle, M. *de la Hire* faw, that fome bits of threads were hardened by this falt, and that the reft were rotten.

VIII.

## VIII. *Of a surprising swiftness in the motion of an almost invisible insect.*

M. *Delisle* has observed, that a fly, so small as to be almost invisible, run over a paper of almost three inches, in half a second. It was so small, that its feet might be reckoned to apply themselves successively upon the whole space that it run over ; and as it appeared to M. *Delisle* that they might be $\frac{1}{3}$ of a line in bigness; it made in the space of a line, fifteen steps, or fifteen motions ; and consequently it made 540 in the space of 3 inches. How nimble must it be to remove one foot above 500 times in $\frac{1}{2}$ a second, or more than 1000 times in one of the common pulsations of our arteries! It is true, that with a magnifying glass, this insect appears to have two wings, but it is not perceived to use them.

## IX. *Observations on some puppies killed hastily.*

M. *Littre* says, that having suddenly, and at one stroke, cut off the heads of sucking puppies, he found the stomachs full of sour and coagulated milk. Now there was not any considerable alteration made in it, since the death of the animal was so quick ; and consequently it appears, that the milk was soured by a natural ferment of the stomach ; and that it is this ferment which makes the digestion, which some skilful moderns make to depend entirely upon the *trituration* of the membranes of this bowel.

M. *Littre* had another design in this experiment ; he had a mind to see if the water of the *Pericardium*, and that of the ventricles of the brain, which we commonly find in dead bodies, were not produced, as some maintain, by the approach

of death, by ficknefs, by agitations, &c. Thefe puppies fo haftily killed, were proper for refolving the queftion. They had water both in the *pericardium*, and in the ventricles of the brain, and confequently they muft have fome natural ufe.

## X. *On windmills.*

The perfection of windmills, that we have boafted of in the hiftory of 1701[*], muft be underftood only of the two points there mentioned. The axis of the mill which carries the fails, muft be put exactly in the direction of the wind, and the fails muft be oblique with regard to this axis, and make with it an angle of almoft 55°. The firft point is always obferved in the practice ; the fecond is often wanting ; and M. *Parent* has obferved, that about *Paris*, the angle which ought to be 55° is 71 ½, which receeds too far from what is prefcribed by the theory of mechanicks.

Befides this well-known defect, which cannot be imputed to the theory, it is very poffible that there may be others in the machine which we do not know, becaufe the geometricians have not examined them enough. They put four fails, and they make them rectangular ; the proportion of thefe rectangles, is commonly 32 feet length of fail, from the centre of the *axis*, or *arbor* ; and 27 feet length of cloth, to 7 in breadth. But how are we fure that thefe are the number of fails, and the figure, and the proportion which agree the beft with the defign of the machine ? We might even confider alfo the pofition of the fails, of which the fmalleft dimenfion, or the breadth, is put to the fide of the *axis*, without any ways doubting whether the other dimenfion fhould not be put there. However, nothing of all this is demonftrated, and M. *Parent* comes at laft to diffi-

[*] Vol. I. p. 258.

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pate this uncertainty, and fix all the views that can be had upon this subject.

The whole force of a windmill depends on the impreffion of the wind upon its fails. We fhall not determine the number of them ; but confider only one, of which the figure, the proportion, and pofition, fuch as we have juft explained, fhall remain undeterminate. We muft only fuppofe the *axis* to be in the direction of the wind, and that the fail makes with the *axis*, an angle of 55°, fince thefe are already two advantages that we are fure of, and two truths, of which we are fatisfied. The impreffion of the wind upon one fail, is as much greater as the furface of this fail is greater, fince there are a greater number of points impelled by a force, which in itfelf, is equal. But as the fail is faftened to the *axis*, the fame wind acts upon the different parts of the fail, with fo much more advantage, as they are farther diftant from the centre or the *axis*, becaufe a greater diftance from this centre, which is the fixed point, is a greater lever, by which the wind acts : thus we may look upon the whole length, or height, of the fail, as a feries of levers continually increafing ; among which there is neceffarily a mean, which fo compenfates the difadvantage of the fmall, and the advantage of the great ones, that if the wind acted by this lever alone, it would act as much as by all the reft together, and therefore the *centre of the impreffion* of the wind upon the fail, is at the extremity of this lever, according to the idea that we have given of thefe forts of centres in the hiftory of 1702, and 1703.

The action of the wind upon the different points of the furface of the fail, being of itfelf

equal,

equal, it is the fame thing as if we fhould confi-
der them as equal weights, but unequally diftant
from a fixed point, and acting by unequal levers.
Now in this cafe, the point where all their action
would re-unite, or to fpeak more accurately, the
point in relation to which the product of the
weights, by their levers, would be equal on both
fides, would be the centre of gravity. Therefore
here the centre of gravity of the fail, faftened to
the *axis*, and the centre of impreffion of the wind
upon the fail, are only the fame thing.

The farther the centre of gravity of the fail
fhall be from the centre of the *axis*, the longer
will the lever of the wind be, and the more ad-
vantageoufly will the wind act. Befides, its force
is abfolute, which is as much greater, as the fur-
face upon which it falls is greater; and confe-
quently the whole force of the action of the wind,
is a product of the furface of the fail, by the di-
ftance of the centre of the *axis* from the centre of
gravity of the fail.

We know, that according to the figure, and
more exactly according to a geometrical nature of
a furface, the centre of gravity is there placed at
different diftances from the fame fixed point, with
regard to which we confider the different levers of
its different parts. Thus the wind may act with
more force upon a fmall furface, than upon a large
one; if in recompenfe it acts upon the fmall by a
longer lever; and in general, the lever of the
wind being variable, according to the figure of
the fails, we muft neceffarily make it enter into
the examinations of the force of the wind. We
fuppofe for the fail, a length, or height, which
fhall always be the fame example, or 32 feet, let
the fail be filled or not, that is to fay, let the cloth
begin, or not begin, from the *axis* of the mill.

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M. *Parent* gave at firſt a figure to the ſail that had never been ſeen; he would have them a ſector of an *ellipſis*; the centre of which, ſhould be that of the *axis*, or *arbor*, of the mill; and the ſmall *ſemi-axis* the height of 32 feet; and for the great, it neceſſarily comes afterwards by the ſuppoſition, which always ſubſiſts, that any ſail whatever is inclined to the *axis* of the mill by 55°. We ſhall not reſt upon this point, which is too geometrical. The elliptical ſail is full: but to have its centre of gravity, it muſt be known what portion of an *ellipſis* it is, if this ſector is ½, or ¼, or ⅙, or which is the ſame thing, if we would allow 2, or 4, or 6, ſails to the mill; and this is the reaſon.

The centre of gravity of an elliptical ſector, is the ſame as that of the correſponding ſector of a circle deſcribed upon the little *axis* of the *ellipſis*. Now to have the centre of gravity of a circular ſector, we muſt have that of the arch of this ſector; and the whole depends upon well underſtanding what the centre of gravity of an arch of a circle is.

We muſt conceive this arch with its cord, in proportion to which all the points of the arch are as equal weights, acting by levers, which are lines drawn from each of theſe points, upon the cord parallel to the *radius*, that cuts the cord in half. The part of this *radius* contained between the cord, and the arch, is the verſed ſine of the arch. This ſine is the greateſt of all the levers; and it is this, by which the ſummit of the arch acts. All the others to the right, and left, continually diminiſh, till they come to nothing. It is viſible, that a mean lever will be ſmaller than this verſed ſine; and will be a part of it; one extremity of which will be at the *vertex* of the arch,

and the other will be the centre of gravity of the arch. The smaller an arch is, the smaller is its versed sine; and consequently also its mean lever, part of this sine, and reciprocally. It remains to see according to what proportion the mean lever is part of the versed sine.

A mean lever among many small, and many great ones, will be as much greater, as the number of the small shall be less, in proportion to that of the great ones. Now in considering the different arches of a circle with their cords, we see evidently, that the greater an arch is, the less is the number of these small levers, and the larger is the number of the great ones. Therefore, the larger an arch is, the greater is its mean lever; and is less exceeded by the versed sine; that is to say, it is a greater part of this sine. The mean lever of the semi-circumference, for example, will be a greater part of the *radius*, which is then the versed sine, than the mean lever of any other arches less than 180, compared in like manner to its versed sine.

The greater an arch is, the greater it is in proportion to its cord, which appears manifestly in this, that an infinitely small arch is equal to its cord; and that therefore the arch of 600, which has the semi-diameter for a cord, is not so great in proportion to this semi-diameter, as the arch of 180; or the semi-circumference, in proportion to the diameter; for these two terms are sufficient to establish the species of the progression. Therefore the greater an arch is in proportion to its cord, by so much is the mean lever of an arch, a greater part of its versed sine.

The greater part of its versed sine, the mean lever of an arch is, the more does the centre of gravity of the arch approach to the centre of the circle, or which is the same thing, the more does the distance of these two centres diminish; and it

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is the smaller in proportion to the *radius*. Therefore the greater an arch is in proportion to its cord, the greater is the radius in proportion to the distance from the centre of the circle, to the centre of gravity of this arch; and it is this exact proportion which is determined by geometry. We see by this, that the distance of the centres of gravity of the arches, from the centre of the circle, varies according to the proportion of the arches to the cords which is continually unequal.

It is easy to imagine what is the centre of gravity of a circular sector. We may conceive them divided into circular zones, which will act with relation to the centre of the circle, by greater or less levers, according as they shall be more or less distant from this centre. They will have a mean lever, of which the extremity that shall be within the circle, will be the centre of gravity of the sector. It is demonstrated, that after we shall have determined the centre of gravity of the arch of this sector, if we take $\frac{2}{3}$ of the distance from the centre of the circle to this centre, it is there that the centre of gravity of the circular sector is.

Since the centre of gravity of a sector depends upon that of the arch, the same consequences recur, the greater a sector shall be, the more will its centre of gravity approach to the centre of the circle; and that in the same proportion, according to which, a greater arch exceeds its cord. Therefore if a power act upon a sector which has the centre of the circle for a fixed and immovable point, and if the whole action re-unites in the centre of gravity of this sector, the power will act by a lever, as much shorter as the sector shall be greater; the same must be said of elliptical sectors, as of circular, since their centre of gravity is the same.

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To calculate the force of the wind upon one fail of a mill, which fhould be an elliptic fector, M. *Parent* has therefore been obliged to determine what this fector is, becaufe of the variation of the centres of gravity ; or which is the fame thing, of the levers of the wind. He at firft took a fector which was $\frac{1}{4}$ of the ellipfis; and confequently has allowed to the mill 4 fails, which would receive all the wind, and not loofe any of it, as the common fails do. Thefe 4 large furfaces multiplied by the lever of the wind upon one of them, exprefs the whole force of the wind to make the machine move, or the force of the machine put in motion.

The fame manner of reafoning applied to a common wind-mill, the fails of which are rectangular, and the height about 5 times greater than their breadth, fhews that the elliptic mill has almoft 7 times more power, which is a prodigious advantage, and certainly deferves to have the common practice entirely changed for it, were it but eafy to change fo common a practice. One would not have expected that the theory could have difcovered fo great an error in an eftablifhed cuftom ; but it is true that elliptical fails would not eafily at prefent themfelves to the mind of the firft inventers.

A mill with 6 elliptic fails would ftill be better for the power, than one with 4. It would have only the fame furface, fince thefe 6 fails would contain the whole fpace of the *ellipfis*, as well as the 4 of the other, but its force would be greater almoft in the *ratio* of 245, to 231. The reafon of this augmentation is vifible. A fector that is $\frac{1}{6}$ of a circle, is lefs than that which is $\frac{1}{4}$ ; and confequently the wind acts upon the firft by a longer lever.

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As the difference of 245, to 231, is trifling ; and as befides the wind might be hindered in fix fails, and be reflected from one to the other, in fuch a manner as would trouble their motion, I believe we had better keep to the mill with 4 elliptical fails.

If we would have it with two, each of which fhould be a *femi-ellipfis*, we fhould find the fame furface alfo ; but the force would be diminifhed almoft $\frac{1}{3}$ with relation to the mill with 6 fails, becaufe the greatnefs of the fectors would very much fhorten the lever of the wind.

Elliptical fails to a mill would be fomething fo new, that the common ufe of them would hardly be adopted. Thus M. *Parent* thought he ought to fearch for the moft advantageous rectangular ones, that is, thofe where the product of their furface, by the lever of the wind, would be the greateft. Every body knows, that the centre of gravity of a rectangle is its middle point, and confequently the lever of the wind is the diftance of this point from the centre of the *axis*.

M. *Parent* infcribes in the elliptical fail of a mill with 4 fails, a rectangle of which he does not determine the dimenfions, and which confequently reprefents all that can be infcribed there, after which, the geometrical rules *de maximis & minimis*, determine this rectangle to be the moft advantageous of all. That which comes by this means, is yet very different from the common ufe. The breadth of the rectangular fail muft be almoft double its height or length ; whereas the height is commonly almoft 5 times greater than the breadth. We fee alfo, that fince we call height, or length, the dimenfion that is taken from the centre of the *axis*, the greateft dimenfion of the new rectangular fail, will be turned on the fide

of this *axis*, quite contrary to the pofition of the old fails. They have been miftaken in all this to a ftrange excefs.

The force of a mill with 4 elliptic fails, would be to that of a mill with 4 new rectangular fails, pretty near, as 23 to 13, which always preferves a great advantage to the elliptic mills.

If we compare together mills with two, with four, and with fix, new rectangular fails, and the moft advantageous that can be, fuppofing this number of fails, and always infcribed in the corresponding elliptic fectors, we fee that thofe which have feweft fails, have moft furface, and leaft force. The force diminifhes becaufe the height which increafes brings the rectangles nearer to the centre of the axis, and confequently alfo their centres of gravity, and fhortens the lever of the wind, according to a greater proportion than the furface increafes. The force of the mill with 6 fails, is to that of the mill with 4, much as 14 to 13, which perhaps ought not to hinder the mill with 4, from being prefered, becaufe of its greater fimplicity. Its force with relation to that of the mill with 2 fails is almoft as 13 to 9.

After this it is eafy to calculate the force of the common mill, where we muft fuppofe the height of the fail always much greater than the breadth. But in this fuppofition, whatfoever proportion the breadth has to the height, let it be $\frac{1}{3}$, $\frac{1}{4}$, or $\frac{1}{5}$, we find always the force of the mill much fmaller than if it had the new rectangular fails, and much more if they had elliptical ones. And alfo the force of the common mill continues to diminifh, juft as its fails are fmaller in proportion to its height, fo that the weakeft of all thofe we have marked, is that where this breadth is $\frac{1}{5}$ of the height, and

yet

yet this is the moſt uſed ; ſo obſtinate does the common practice ſeem in being miſtaken.

The uſefulneſs of all this theory of M. *Parent's* is eaſily perceived. A ſtronger mill will turn faſter, and with a leſs wind, and will diſpatch more work. In a low place, and where for want of wind it would be uſeleſs to conſtruct an old mill which would be too weak, they may conſtruct a new one. We ſhall have, if we pleaſe, a mill of a leſs height of ſail, which, nevertheleſs will be equal in force to an old one ; and we ſhall know exactly how much we ſhall lower its ſail ; preſerving this equality. When we ſhall know the effect that is required to the mill, that is the force that we would give it, in pounds, it will be eaſy to find by calculation the number of ſails, their figure, proportion, and even the varieties, and different combinations, that theſe things may have always the ſame effect. The trouble of diſcovering the true principle is always rewarded by a great number of eaſy conſequences.

XI. *On the manner in which ſeveral ſorts of ſhell-fiſhes faſten themſelves to certain bodies.*

In treating of the progreſſive motion of ſeveral kind of ſhell-fiſh, in the hiſtory of 1710\*, we have ſpoken of the almoſt perpetual immobility of ſome of them ; for we cannot treat of their progreſſive motion without ſaying that the greateſt part of them have hardly any ; and in this reſpect partake more of plants, than of animals. There are even ſome which abſolutely never ſtir from the place where (if I may uſe the expreſſion) they

\* Vol. III. page 321 of this abridgment.

have taken root. We are at prefent going to explain their immobility according to M. *de Reaumur*, the author of all thefe obfervations.

The *patella*, or limpet*, fixes itfelf by a very flat bafe to ftones, and even very fmooth ones; and is faftened there fo ftrongly, that being put in a fituation where this bafe and the ftone were vertical, it required a weight of 28 or 30 lb. to make it let go. We muft obferve that this bafe, which is elliptical, has but one inch in its greateft diameter. From whence can this great ftrength proceed? it is not very probable, feeing the fmoothnefs of the two bodies, that the bafe of the limpet, how mufcular foever, fhould be fufficiently inferted into the imperceptible inequalities of the ftone; and indeed this infertion would not have any great effect in the vertical fituation. Befides, M. *de Reaumur* is affured by decifive experiments, that this fhell-fifh faftens itfelf fo ftrongly to the ftone, by the means of a glue which proceeds from it; and that the action of the mufcles of its bafe, which we might add thereto, has no fhare in it.

This glue is yet more remarkable in the fea-nettles*, which are not lefs clofely fixed to the ftones. Thefe animals are neither covered with fcales nor fhells, and their fkin is not a membrane, or texture of folid fibres; it is only a covering of glew, which diffolves very foon in fpirit of wine, whilft the reft of the body of the animal remains entire, and without alteration.

The 1520 legs of the ftar-fifh* do not feem to be given it fo much for walking, as for not walking. They are very foft, and ferve it to ftick to the neighbouring bodies, in fuch a manner, that

* Vol. III. page 321 of this abridgment.

if

if one had a mind to difengage it from them, we fhould only break them.

The fea-mufcles have a much more fingular manner of faftening themfelves. They throw out fome threads of the bignefs of a thick hair, at moft about 3 inches long, and fometimes to the number of 150, with which they feize all that furrounds them, and generally the fhells of other mufcles. They are thrown on all fides; and they hold by them, as by cords of different directions. M. *de Reaumur* has not only feen that they fpin them, and that when they have been cut off, that they have fpun others; but he has difcovered the curious particulars of the mechanifm that they make ufe of.

The *pinnæ marinæ*, another fort of fhell-fifh, fix themfelves alfo in another fituation, by threads much finer than thofe of the mufcles, but in a much greater number; there are fine works made of them, whereas thofe of the mufcle are good for nothing. There are no *pinnæ marinæ* on the coafts of *Poictou*, where M. *de Reaumur* has obferved; but it is a ftrong prefumption that they fpin alfo. Thefe will be the filk worms of the fea, and the mufcles will be the caterpillars.

Laftly, the worms which are commonly called worm-fhells, becaufe being otherwife pretty like the earth-worms, they are inclofed in a round tube, of the fubftance of a fhell, make themfelves a habitation which they never quit, by fixing their tube on a ftone, or hard fand, or fome other fhells. This tube exactly follows the circumvolutions of the furface where it is glued, rifes, or finks with it, *&c.* It winds alfo without being obliged to it by this furface; and becaufe it feems to have followed the natural motion of the worm. All this explains itfelf in M. *de Reaumur*'s fyftem,

who

who pretends that this tube, as well as the shells of the snails, is formed of a glutinous matter which comes out of the body of this little animal.

Another sort of sea-worms, which probably transpire less of this matter, make their tube only of grains of small sand, and little fragments of other shells which they unite together by their glue; and this little building of pieces brought together, is however neatly enough made.

It is by means of this very glue, that the oysters stick themselves to the rocks, or to one another; and lastly, this is the universal cement which nature uses whenever she has a mind to build in the sea, or to secure any thing there against the perpetual and violent motion of the waters. The most simple means properly employed are the most efficacious.

### An explanation of the figures.

*Plate* II. *fig.* 1. represents a *patella*, called in *English*, a *limpet*, *flither*, or *papshell*, fastened to a stone. It shews only the shell which covers it. The letters BBB mark the circumference of the base of this shell; S is its *vertex*. There are different flutings which go from the *vertex* S to the base.

*Fig.* 2. is a *limpet* detached from the stone, and put into a reversed position T; is the head of the animal; CC are two horns placed near the head; P is the fleshy base of the animal. It is this base P that is applied to the stones, and sticks to them. Its surface seems rugged, and as it were shagreened. All these inequalities are formed by an infinite number of different little vesicles.

*Fig.* 3. is a sea-muscle represented open. The muscle MM, which serves to shut the shell, has been cut.

AB

AB is a part of the mufcle which refembles the tongue of an animal. It is the inftrument that moulds the threads formed by the animal. A is the origin, bafe, or root of this inftrument. B is its point, or extremity.

AI is a ftreak, or rather cleft, of which the two edges are applied to each other, and it forms a canal on the infide. This cleft divides the fpining inftrument into two equal parts. From A to I we muft obferve fome circular, or rather tranfverfe fibres: they ceafe in I.

AF is a part of the bundle of threads which ferve to faften the mufcle; thefe threads have been cut at F, that the figure might be lefs confufed; and alfo to fhew how fhort they were, when we had cut them with a pair of fciffars, introduced into the fhell on account of this figure 3; we fhall obferve, that the mouth of the mufcle is at C; it is formed of two pretty thin membranes, which feem to be applied to each other. We do not fee this mouth open, unlefs we take care to open it: its breadth is HH. This mouth is a fort of funnel, very much flatted, and terminates in a duct, which reaches quite to the *anus*. It is nourifhed in all probability only by the water and earth; its excrements are of the colour of the mud.

*Fig.* 4. is a mufcle, which having prolonged its fpinning inftrument, marked at prefent LI, feels about to find the ground before it fixes itfelf. This inftrument appears under a very different form from that which it has in the inaction, as may be feen at AB, *fig.* 3.

VVV are *tubuli vermiculares*, or *worm-fhells*, ftuck upon the fhell of a mufcle. Thefe worms grow indifferently upon all forts of bodies, as ftones, fand, and other forts of fhell.

*Fig.*

*Fig* 5. is compofed of 1. a mufcle G, which is faftened to a ftone by different threads, DDD, &c. The bafe DD of thefe threads, is three or four times more in diameter, than the reft of the thread. We fee at G a little end of the tendon, or big thread, to which all the flendereft threads are faftened. 2. In *fig.* 5. there is a mufcle N, which after having darted the 2 threads NQ, NQ, actually darts a third NT. T is the place where the end of this thread ought to be ftuck. We may obferve, that the fpinning inftrument is thicker there than towards the point; and that it forms a fort of heel.

*Fig.* 6. is the half of a mufcle, where the fpinning inftrument is however left entire. We fee there two of the four mufcular ligaments, which hold it. RS is one of thofe which faften it toward the *vertex* at S. ZX is one of thofe which faften it toward the bafe at Z.

*Fig.* 7. is a fpinning inftrument detached. KP is the cleft, or canal, in which the liquor which becomes thread, paffes. This canal ceafes at P, the part PO, where it does not reach, is thinner than the reft of the bafe. At the root K of the fpinning inftrument there appears a hole K, which is the refervoir, wherein the liquor is gathered that rifes in the fpinning inftrument, in the fame hole K is lodged one of the ends of the tendon, or big thread of *fig.* 9. to which all the flender threads are faftened.

*Fig.* 8. is the fpinning inftrument feen behind. There are two pieces of mufcular ligaments MM, which ferve to faften it. Thefe pieces are parts of the ligaments, fuch as the ligament marked ZR, *fig.* 6.

*Fig.*

*Fig.* 9. AB is the tendon, or big thread, to which all the reft are faftened, as the figure reprefents. In feveral mufcles it is much fhorter than it appears here; but there are fome where it is bigger. Its extremity A is faftened in the hole K of *fig.* 7. or as it is feen at A, *fig.* 3. All the threads, which the mufcles have formed with me, were faftened near A, which inclines me to believe that this tendon, or big thread, grows like our hairs; and that the flender threads, which at firft were faftened at A, are faftened at B by the croffing of big threads.

*Fig.* 10. is a mufcle reprefented in the ftate in which it is when it refpires the water. CD is the aperture by which it refpires. The canal by which it throws out its excrements, opens into the fame aperture CD. The outlet of this canal, or the *anus* of the mufcle, is at C : the excrements, which come out of it, feem to be meer earth, a fort of mud. They have a fluting thro' their whole length; I mean that they are made as a portion of a hollow tube. Thence it is evident, that the canal by which they are difcharged, or at leaft the aperture thro' which they pafs, is not round as in other animals. RH is the place, or fpring, which ferves to open the fhell. EE are an infinite number of little flefhy parts very prettily cut, refembling little cocks-combs. The animal fhews only when it refpires the water, they are feen alfo at EE. *Fig.* 5. the refpiration is not ftopped while it darts the thread.

*Fig.* 11. is one of the pieces of which the fhell of a mufcle is compofed; we may obferve a little band which covers the inner edge of the fhell. This band is of a horney kind of fubftance, and is faftened in its natural ftate to the circumference of the body of the animal.

<div align="right">*Fig.*</div>

*Fig.* 12. is a *pectunculus,* or *cook-fish,* . faftened to a ftone by different threads FFF, the *vertex* of the fhell is at S; on both fides of S, is the fpring which ferves to open the fhell; for this fhell is bivalve, like that of the mufcle. There appear feveral flutings, which go from the *vertex* S, to the bafe BB. In different parts the fhell is ftuck with points; fo is that point of the fhell which is called the ear.

*Fig.* 13. is a cook-fifh reprefented open. The thick mufcle MM, which ferves to fhut it, has been cut. L marks the *vertex* of the fhell, and the middle of the fpring which tends to open the fhell. T and R are two appendages, which being placed one above the other, form the ear. The appendage T is narrower than the appendage R, fo that the firft does not entirely cover the fecond. They do not apply themfelves fo exactly to one another, as not to leave a little opening by which a part of the threads came out, which are feen in *fig.* 12. HG is the fpinning inftrument of the cook-fifh; GP is the bundle of threads: thefe threads have been cut fhort at P, for fear of rendering the figure confufed. They are all faftened to a common tendon at P, which is faftened to the origin of the fpinning inftrument.

*Fig.* 14. is a cook-fifh reprefented in fuch a pofture as to fhew the canal VX, thro' which the excrements of the animal pafs. X is the aperture of this canal, or the *anus* of the cook-fifh.

*Fig.* 15. is a heap of fand, in which a great number of *worm-fhells* were lodged. On the upper furface of this heap of fand is the out-let of all their tubes; and on one of the fides, as at BC, may be diftinguifhed, the length, roundnefs, and curvature of thefe tubes.

*Fig.* 16. is one of the sand worm-shells, reprefented almost in its natural bigness.

*Fig.* 17. is the same worm seen with the microfcope. The extremity of the head is the flat surface feen at T. This extremity, the circumference of which is round, is sometimes made like a horse-fhoe, when the animal opens in O. NNN are the fins of the worm. HH, II, EE, are three rows of little flefhy hooks. Q is the tail of the worm.

## XII. *On a new purple.*

There are not only more things found in these laft ages than there have been old ones loft, but there can hardly be any thing loft that we are not contented fhould be fo. For in fhort we need only fearch in the bofom of nature, where nothing is annihilated ; and it is alfo a great ftep toward recovering it to be fure that it may be found. The purple colour, formerly fo much in efteem, that it made among the *Romans* one of the principal marks of dignity, either has not been abfolutely loft, as fome think, or at leaft has been recovered within thefe 30 years, by the royal fociety of *England.* One of the fhell-fifhes which furnifhes it, and is a fpecies of *buccinum,* or *whelk,* is common on the coafts of that country.

Another *buccinum* which alfo gives the purple tincture, and which probably is one of thofe which *Pliny* has defcribed, as having that ufe, is found upon the coafts of *Poiétou,* and M. *de Reaumur* having a mind to ftudy it particularly, difcovered another purple which he did not feek for, and which in all probability was unknown to the ancients, though of the fame fpecies with theirs.

The *buccinums* of *Poictou* which yield the purple, are commonly found gathered about certain stones, or sand, covered with oval grains, about 3 lines long, and a little more than a line thick, full of a white liquor, a little yellow, pretty much like that which is drawn from the *buccinums* themselves, and which after some alterations, acquires the purple colour. By M. *de Reaumur's* experiments it is not likely that these grains are the eggs of the *buccinum*, nor are they the seeds of any sea-plants; they must therefore be the eggs of some fish. They do not begin to appear till autumn.

These grains crushed upon a white cloth, at first only make an almost imperceptible yellow, but in 3 or 4 minutes they give it a very fine purple red, provided that this cloth be exposed to the open air: for what is very remarkable, and which shews of what extreme delicacy the generation of this colour is, the air of a room, even though the windows should be open, would not be sufficient. The tincture of these grains fades a little by a great many washings.

M. *de Reaumur* has discoved by some experiments, that the effect of the air upon the liquor of the grains, consists not in its carrying off some of its particles, nor in giving it new ones; but meerly in agitating it, and changing the order of the parts which compose it. We have in the cochineal a very fine red colour, but good only for wool; and is of no use, either for silk, or linnen. The *carthamus*, or *saff-flower*, gives a fine scarlet and crimson, but it is only to silk. We may find in cultivating the grains of M. *de Reaumur*, the fine red that we want for linnen, which perhaps will surpass the red of the *Indian* linnen which is not fine.

M

Pl. II. Vol. IV.
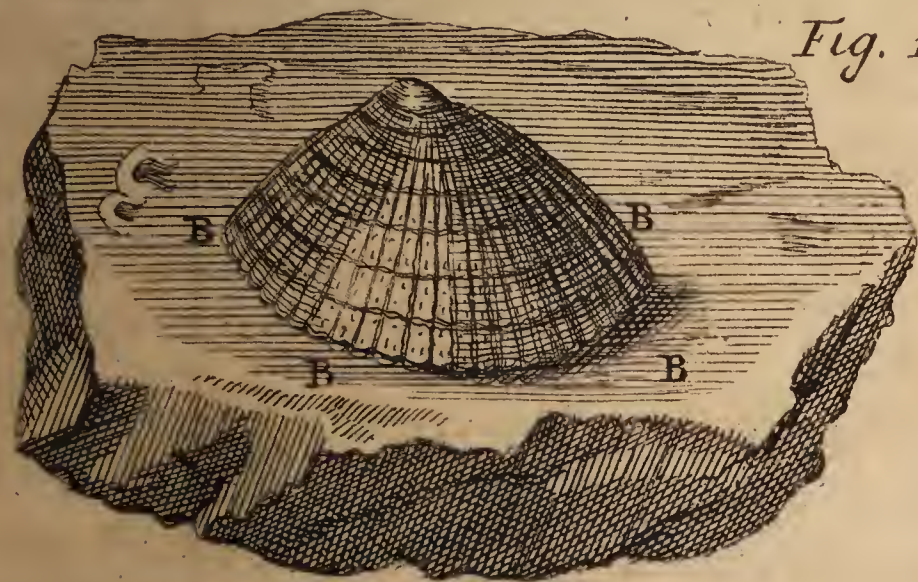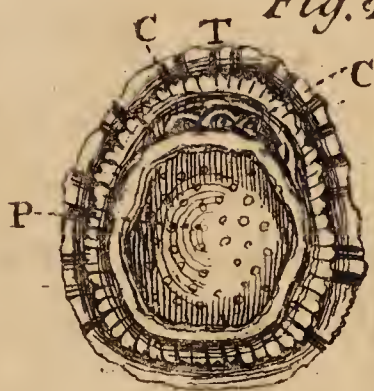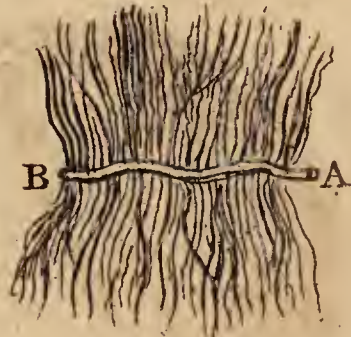


Fig. 1. Fig. 2. Fig. 9. Fig. 10. Fig. 11. Fig. 3. Fig. 4. Fig. 12. Fig. 13. Fig. 5. Fig. 6. Fig. 15. Fig. 8. Fig. 14. Fig. 16. Fig. 17. Fig. 7.
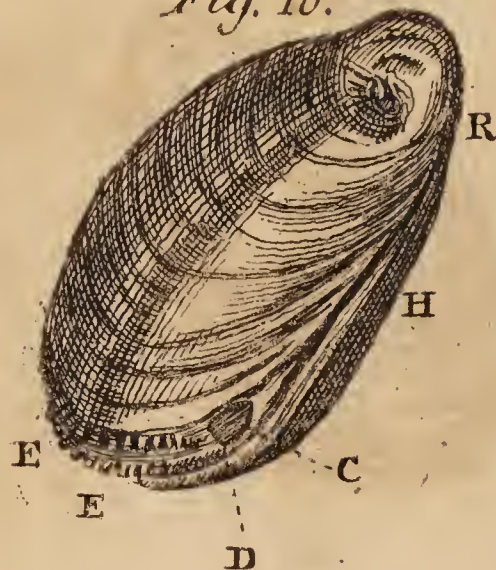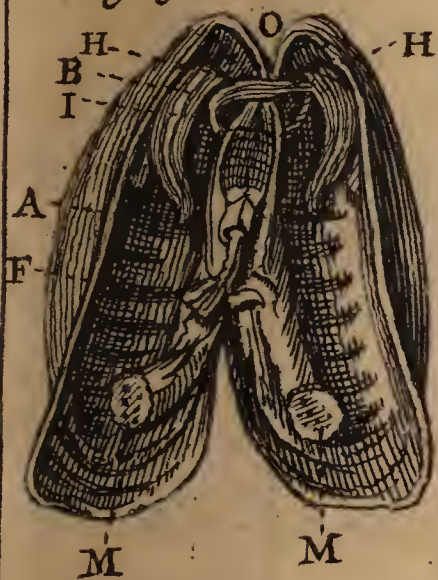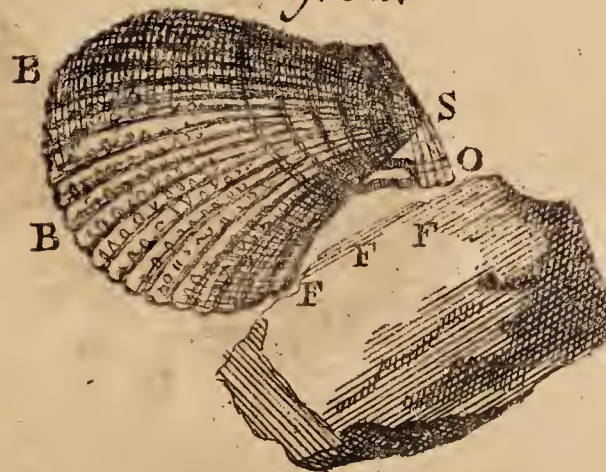
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furpafs the red of the *Indian* linnen which is not fine.

M. *de Reaumur* has not failed to compare his new purple with that which is drawn from thefe *buccinums* of *Poiɛtou.* The *buccinums* have at their collar, for we may allow them to have one as well as the fnails, a little refervoir, called improperly by the antients, a vein, which only contains one great drop of yellowifh liquor; the linnen that is dyed with it being expofed to a moderate heat of the fun, acquires at firft a greenifh colour, afterward a lemon colour, a brighter green, then a little deeper, from that a violet, and at laft a fine purple. This is done in a few hours; but if the heat of the fun be very ftrong, the firft changes are not perceived, and the fine purple appears all at once. A great fire produces the fame effeɛt, only a little flower, and does not produce fo perfeɛt a colour. Without doubt the heat of the fun being much more fubtle than that of a wood fire, is moft proper to agitate the fineft particles of the liquor. The open air aɛts alfo, tho' lefs quick, upon the liquor of the *buccinums*; efpecially if it is diluted with a gcod deal of water; from whence M. *de Reaumur* conjeɛtures, with great probability, that the liquor of the *buccinums*, and that of the grains, are almoft of the fame nature, except that this of the grains is more aqueous. They differ alfo in tafte; that of the grains is falt, and that of the *buccinum* extremely hot, and biting, perhaps becaufe it is lefs diluted with water.

If we would make ufe of them in dying, that of the grain would be much more convenient, and would coft lefs; becaufe it is very eafy to draw

it from a great number of grains bruifed all at once; whereas to have that of the *buccinum,* we muft open the refervoir of each particular *buccinum,* which requires a great deal of time; or if for expedition we bruife the fmalleft of thefe fhell-fifh, we fhall fpoil the colour by the mixture of different matters which the animal furnifhes.

We might find perhaps fome chymical liquors which would make the purple colour appear quicker, or more conveniently, than the fire, or the fun, or the open air; and M. *de Reuamur* has already found the corofive fublimate, which produces this effect on the liquor of the *buccinums*; but practice, and above all, a practice which fhould come to make part of a trade, would require a great many other obfervations, and quite new intentions. There is a vaft difference between a philofopher who aims at knowledge, and a mechanic who aims at profit.

### *An explanation of the figures.*

*Plate* III. *fig.* 1. reprefents a ftone DHHFEED, in which is feen a great number of thofe little grains which M. *de Reaumur* calls eggs of the purple, faftened as to an arch againft one of the faces of this ftone. This face was downward, but it did not touch the fand. GGG, *&c.* are different places where thefe eggs of the purple are faftened. The eggs of the purple marked EE, are ftuck upon other eggs, as the other eggs are ftuck upon the ftone.

*Fig.* 2. is that of an egg of a purple; p is its bafe, the extremity of its pedicle; it is this extremity that is ftuck againft the ftone. p r is its pe-
dicle;

dicle ; this pedicle p r fuftains a little bottle r b ftopped at b, with a ftopple b. d d mark the thicknefs of this bottle.

*Fig.* 3. is alfo an egg of a purple, where the fame letters mark the fame parts as in the preceding figure ; but b fhews the ftopple loofe ; o the aperture of the bottle in which the ftopple was.

*Fig.* 4. is a large reprefentation of an egg of a purple. The letters PRB fhews the fame things as the letters p r b in *fig.* 2 and 3. The letters III fhew the manner in which the different drops of yellowifh liquor are diftributed in the middle of the clear liquor. As the fides of the egg are tranfparent they difcover thefe different liquors.

*Fig.* 5. is a little *buccinum*, or *whelk*; it fhews the aperture of the fhell ; and at the edge of this aperture are feveral flutings o o o o.

*Fig.* 6. is a whelk, the fhell of which differs from the preceding by the coloured ftripes RR.

*Fig.* 7. is the fame whelk as *fig.* 5. which fhews its head. T is the head, at the fides of which are 2 horns CC. DDDD mark that part of the fhell which covers the collar, or upper part of the back of the animal. This part DDDD of the fhell muft be raifed to perceive the little veffel wherein the purple dye is contained.

*Fig.* 8. is the fame whelk with *fig.* 7. with a piece of the fhell DDDD removed, which fhews its collar EEEE. Upon this collar, or if you pleafe, on the back of the animal, appears the little veffel v v. It is in this veffel that the purple dye is contained.

*Fig.*

*Fig.* 9. is that of the whelk from which *Columna* pretends the purple dye of the ancients was taken. This whelk ufually fhews 3 horns, the largeft of which C is in the middle between the 2 fmaller c c. The fame whelk may be feen in the hiftory of 1710, with the great horn C in a different pofition.
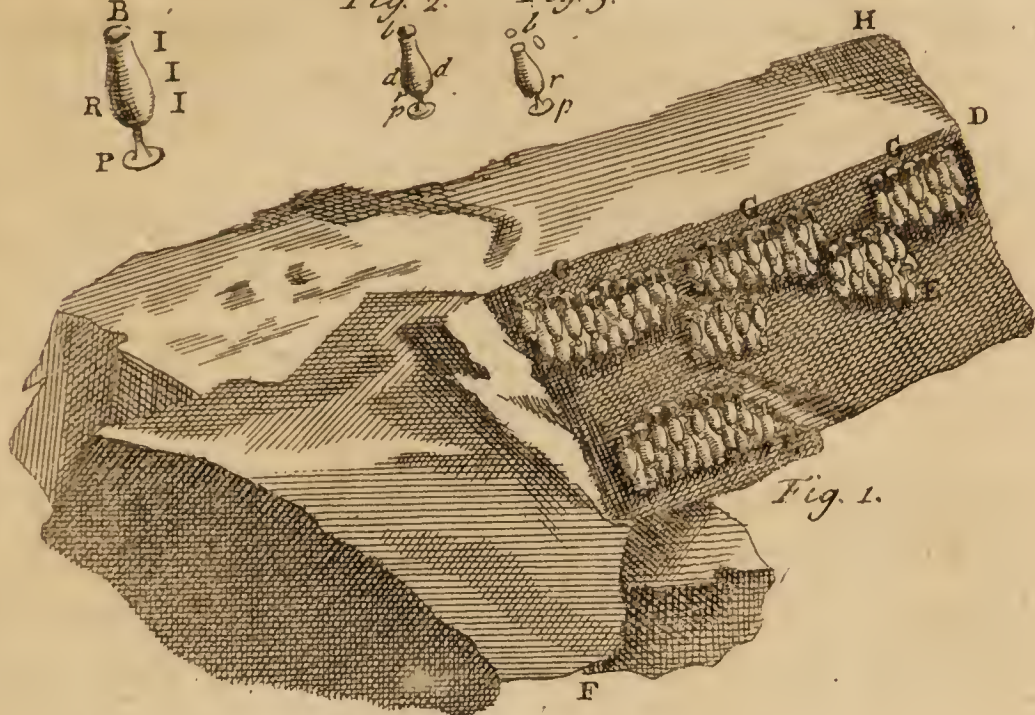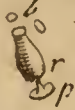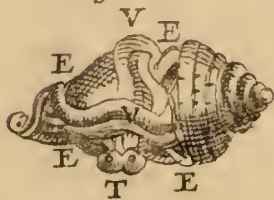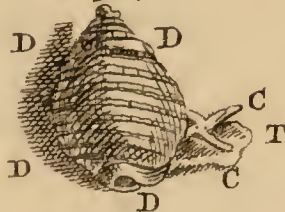
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Pl. III. *Vol. IV.*                                                                p. 116.

# AN
# ABRIDGMENT
## OF THE

PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1711.

I. *Obſervations on the height of the water which fell at the obſervatory during the year 1710, with thoſe on the thermometer and barometer, by M.* de la Hire*.

THESE are the obſervations of the quantity of water, and melted ſnow, that fell at the obſervatory during the whole year of 1710, which have been made in the ſame manner with thoſe of the preceding years. In

| | Lin. | | | Lin. | |
|------|----|-----|--------|----|-----|
| Jan. | 12 | 3/8 | July | 17 | 3/8 |
| Feb. | 3 | 1/2 | Aug. | 37 | 3/8 |
| March | 14 | 1/8 | Sept. | 15 | 3/4 |
| April | 17 | 3/4 | Oct. | 11 | 3/8 |
| May | 12 | | Novem. | 21 | 1/8 |
| June | 9 | | Dec. | 17 | |

The ſum total of water of the whole year, 188 lines 3/4, or 15 inches 8 lines 3/4.

Theſe obſervations ſhew us that the year 1710 has been one of the drieſt that we have had a great while, in compariſon to the 19 inches of water which commonly falls. The year however has been very fruitful in grain, as it always happens in

* Jan. 9, 1711.

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thefe countries, becaufe the greateft part of the grounds are cool and moift.

There fell no fnow at the end of the year, but at the begining of it, about the middle of *January*, it fnowed moderately ; which gave me the opportunity of making the following experiments.

The 10th of *January* in the morning I covered the ball of my thermometer, which is always expofed in the open tower of the obfervatory, with a very great quantity of fnow, and after having left it there full 3 hours, I did not obferve that the fpirit of wine had changed its height in the tube; it then remained at 27 parts, and it begins to freeze in the country when it is at 32, by which we fee that the air was not much colder, that in the beginning of the froft ; and altho' the thermometer always rifes from the morning 'till noon, and afterwards it does not change its height for 3 hours, becaufe. the degree of cold of the fnow kept it always in the fame ftate, the fmall increafe of heat of the air, not being capable of penetrating in fo fhort a time, the mafs of fnow which was about the ball.

But the air being extremely cool till the next day, the 11th of this month, this thermometer being then at 14 ½ parts, which marked a great cold, I repeated the experiment of the preceding day, and there happened the fame thing, the thermometer remaining at the fame height, in the fnow as it was out of it; from whence I conjecture that the cold of the fnow is not a cold that is proper to it, but that it only takes the degree of cold of the air as it then is, becaufe it is rare enough to leave the air at liberty to infinuate itfelf by degrees into all its parts ; thus the fnow will contribute nothing to the cold, except the pre-

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ferving the coldnefs of the air for fome time in the fame ftate.

There was not any thing confiderable to remark upon the winds, except that *October* 11th, there was a fort of hurricane, the wind being at S. S. W. without rain.

The thermometer marked the greateft cold of the year, *January* the 11th, being fallen to $14\frac{1}{2}$ parts, which marks a great cold ; but the 12th it rofe again to 27, where it was the 10th and from that time the cold was moderate.

As for the heat, it was alfo moderate during the whole fummer, the greateft was mark'd by the thermometer at 61 parts the 3d of *Auguft* at fun-rife, and at $2^h \frac{1}{2}$ P. M. the thermometer was at $71\frac{1}{2}$ parts ; thus the cold was greater than the heat, in proportion to the mean ftate, where it is at 48, but it lafted only one day, as I have juft related.

My common barometer which is always placed at the top of the hall of the obfervatory, was at the higheft at 28 inches 3 lines $\frac{1}{6}$ *January* the 3d. with a S. W. which is very extraordinary, for it is commonly rather low than high, when the wind is in the fouth. It was at the loweft, *March* the 7th, at 26 inches, 10 lines $\frac{2}{6}$, alfo with a fouth-wind, and rain. The difference between the higheft and the loweft was therefore 1 inch, 4 lines $\frac{1}{3}$ a little lefs than ordinary, which is 1 inch 6 lines.

I alfo obferved that in the whole month of *February*, when it rained but very little, the barometer was very high as it commonly is ; it was alfo the fame in the firft half of the month of *September*.

I here again give notice, that when the obfervations are made, they fhould take care to ftrike a little againft the wooden frame, where the tube

*is*

is fixed, to make the quickfilver run to its true height ; for as it always adheres a little to the infide of the tube, it does not move freely, and fometimes there is found ½ a line difference between the height at which it appears to be at firft, and the true height where it ftops at, efpecially if the tube be thin.

*December* 30, I obferved the declination of the needle of 8 inches long, in placing the fide of the box againft the fame ftone pillar where I geneally put it, and I found it 10⁰ 50' towards the weft.

II. *A comparifon of our obfervations on the height of the rain-water, and on the barometer, which thofe which* M. Scheuchzer *made at* Zurick, *in* Swizzerland, *during the year* 1710, *by* M. de la Hire†.

M. Scheuchzer has fent us this year his obfervations of the rain water, and of the barometer and thermometer, which he has made at *Zurick*, as in the year 1709. He found the height of water only 23 inches ¾, during the whole year 1710, and he adds that it is lefs than the preceeding year, by 9 inches 2 lines ¼, and yet this little height is greater than any of the greateft that we have obferved at *Paris* fince the year 1699.

I have related in the memoir of the preceding year, my conjectures upon what may caufe this greater height of water in the mountains, and therefore I fhall fay no more of them here. We fhall only obferve that the height of the rainwater at *Paris* in 1710, was only 15 inches, and

† Aug. 5. 1711.

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almoft 9 lines, which is much lefs than ordinary, and lefs than the preceding year by 6 inches; which agrees in fome meafure with M. *Scheuchzer's* obfervations; and fhews that at *Zurick*, and at *Paris*, the year has been dryer than ordinary.

He adds, that the greateft height of his barometer was *Jan.* 3, at 26 inches, 9 lines $\frac{3}{4}$, and the leaft at 26 inches, o lines $\frac{1}{2}$, *Dec.* the 25; the difference is therefore 9 lines $\frac{1}{4}$.

I found my barometer alfo at the higheft the 3d of *Jan.* at 28 inches, 3 lines $\frac{1}{8}$; therefore the difference of the height of the quickfilver the fame day at *Zurick*, and at *Paris*, was 1 inch, 5 lines $\frac{5}{12}$, or 17 lines $\frac{5}{12}$; from whence we might conclude pretty near, how much higher *Zurick* is than *Paris*, if our barometers agreed.

The leaft height of quickfilver which I found, was 26 inches, 10 lines $\frac{5}{6}$; therefore the difference of our leaft heights will be 10 lines $\frac{2}{3}$, which is very different from the preceding; the days were alfo very different; and the 15th of *Dec.* which was the day of the obfervation at *Zurick*, my barometer was at 27 inches.

As for the heights of his thermometer, I cannot compare them with mine, for they muft have been rectified by one another.

III. *Experiments to know whether the ftrength of cords exceeds the fum of the forces of the threads which compofe them, by M. de Reaumur* [*].

We are prejudiced in believing, that a cord compofed of different threads twifted together, has a force which furpaffes the fum of the forces

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[*] *Feb.* 21, 1711.

of all the threads which compofe it. I mean that if, for example, we make a cord with 6 threads, each of which will bear a weight of 5 lb. without breaking, it is generally thought, that that the cord made with thefe 6 threads, will bear a weight above 30 lb. feveral learned men are agreed with the vulgar in this, as I had the opportunity of feeing, by the objections which were made to me by fome of the moft illuftrious perfons of the academy, upon a paffage in the memoir where I examined the filk of fpiders. This paffage treated of the ftrength of the filaments of filk. A fkilful geometrician pretended even to have the demonftration of the proportion, in which the twifting is convenient ; and I fhall often have occafion hereafter for the proportion, in which the twifting increafes the ftrength of the cord above the fum of the forces of all its threads.

It feemed to me on the contrary, that it was without having examined the thing clofe enough, that it has been imagined that the twifting increafes the ftrength of cords, that every thing being well confidered, we fhall find perhaps, that far from increafing, it diminifhes it; and that this is one of thofe phyfical problems which cannot be refolved but by phyfical experiments. Befides I thought it would be of fome ufe to mechanicks to endeavour to refolve them. We might often expofe the cords that we make ufe of, to be broken, if we fhould reckon too much upon their ftrength.

All that they do in making of cords, or in twifting the threads about one another, is to put them all in a ftate of contributing fomething to fuftain the force, or the weight that is made to act

againft

against this cord ; and at the same time to dispose each thread in such a manner, that it is easier to break it, than to make it slip, or to disengage it from those which surround it. It is this which gives the facility of making very long cords with very short threads, as we see in the cords of hemp, flax, wool, and silk, for we may look upon the filaments of silk, and wool which they commonly make use of, as little cords; each thread being pressed against those that surround it, and being twisted with these threads, opposes by its friction such a resistance to the force which draws it, that it is more difficult for this force to overcome the resistance of the friction, than to break the the thread.

But does it follow from this disposition of the threads, that the sum of their forces is smaller or greater, than the forces of the cords are ? It is not possible to decide this by reasoning alone. We see plainly that in twisting several threads together, we shorten each thread, and that the cord gains in thickness, what each loses in length ; if we regard the cord only on that side, it is evident that its force is increased. For every thing else being equal, the thickest cords are the strongest. If for example we make a cord by twisting 5 threads about one another, and that the twisting shortens each thread $\frac{1}{5}$, it is evident that the thickness of the cord gains $\frac{5}{5}$ by which the length of the threads are diminished ; from whence it seems evident, that the strength of this must be equal to the sum of the forces that 6 threads would sustain separately.

There is also another way, in which the twisting seems to augment the strength of the cord ; it is because the weight, which draws the cord, draws each thread obliquely ; so that one part of

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this weight is employed to prefs thefe threads one against the other ; being each lefs drawn according to their length, the cord which they compofe might be in a ftate of refifting a greater effort, than that which all the threads that compofe it can fuftain, when they are drawn perpendicularly.

These are the favourable fides on which we can view the twifting: but on other fides we fhall fee that it weakens the force of the cords, if we would endeavour that in order to a cord's having a force equal to the fum of the forces of the threads which compofe it, the weight faftened to one of its extremities, fhould act againft each thread only in proportion to the force of this thread. For if the weakeft threads are charged as much as the ftrongeft, or if fome threads of equal ftrength are much more charged than others, they will break, and the weight will fall upon the threads, which before were the leaft charged. Now the weight that draws a cord, draws each thread that compofes it, more or lefs, in proportion as this thread is more or lefs ftretched, and more or lefs thick ; and in twifting thefe threads it is impoffible to difpofe them in fuch a manner, that the weakeft fhould be lefs ftretched than the others; fometimes the thickeft are the weakeft ; each thread therefore does not contribute in proportion to its ftrength to fupport the weight; and if for example, in a cord compofed of 6 threads there are 4 which contribute only half their ftrength to fuftain the weight, the cord muft be only confidered as if it was compofed of 4 threads.

Befides fince in twifting the threads, they are ftretched ; it is plain that the twifting is equivalent to a weight which would draw each thread, and to a weight greater or lefs according as the tenfion that it produces is greater or lefs ; that is,

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the more this thread is ftretched, the lefs it is in a ftate to fuftain a weight equal to that which it would fuftain naturally. The twifting alone is fufficient fometimes to break the threads, as we find by experiment, when we fee them too hard twifted.

The fame twifting, which increafes the ftrength of the cords in fome places, diminifhes it in others. But does the increafe furpafs the diminution? here geometry has no effect any further than we make arbitrary fuppofitions, which confequently determines nothing : we cannot know whether among thefe fuppofitions we have chofen thofe which are conformable to the effects of nature ; we muft therefore here, as in all other philofophical doubts, have recourfe to experiments ; thofe under confideration are fimple and eafy to execute. I fhall relate exactly a part of thofe that I have made ; they will teach us what we are to think of the increafe of the ftrength of cords, above that of the fum of their threads.

*Exp.* I. I took a bottom of white thread, fuch as is commonly ufed, and having cut off a great piece of it, I fixed at one end different weights, from 1 pound to 10; this bit of thread fuftained 9 pound $\frac{1}{2}$ without breaking, and it broke when I had fixed to it a weight of 10 pound. It was therefore evident that each of the two parts which remained after the divifion of this thread, could at leaft bear a weight of 9 $\frac{1}{2}$ pounds, fince they had already fuftained it without breaking. I afterwards doubled the longeft of thefe ends of thread, and twifting the two peices together, of which this thread confifted, I formed a little cord compofed of two threads, each of which was able to bear 9 pound $\frac{1}{2}$, confequently if the twifting had increafed the ftrength of the cord,

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above the fum of the ftrength of the threads which compofe it, this cord ought to have born above 19 pound; it was very well twifted without being too much fo. Neverthelefs this cord broke when I had fufpended a weight of 16 pound to it, and it only fuftained 15 pounds $\frac{1}{2}$ without breaking; fo far from its ftrength being increafed by the twifting, it was diminifhed about $\frac{1}{6}$.

*Exp.* II. I afterwards fixed a weight of 6 pounds $\frac{1}{2}$, to another thread taken from the fame bottom, it fuftained it without breaking, and broke with 7 pound, I alfo fixed feveral weights to 2 other threads the firft of which refifted a weight of 8 pounds, and broke at 8 pounds $\frac{1}{2}$, and the fecond fuftained 8 pounds $\frac{1}{2}$, and broke at 9. I took the longeft end of each of thefe 3 threads, and twifted them into a fmall cord of three threads: the fum of the forces of thefe three threads was at leaft capable of fuftaining a weight of 23 pounds. The cord neverthelefs broke when it was charged with 17 pounds $\frac{1}{2}$, the twifting has therefore confiderably weakened it.

*Exp.* III. Having likewife taken 4 bits of thread, and knowing by the experiments, that the firft could fuftain 8 pounds $\frac{1}{2}$, and that it broke with 9; that the fecond could fuftain 6 pounds $\frac{1}{2}$, and broke with 7, and that the other two had born 7 pounds, and broke with 7 $\frac{1}{2}$; I made a cord by twifting thefe 4 threads, I knew by the experiments juft mentioned, that the fum of the forces of thefe threads could at leaft fuftain a weight of 29 pounds; I therefore eafily knew that the ftrength of this cord was lefs than that of the fum of the threads, when I faw it break after having hung to it a weight of 21 pounds $\frac{1}{2}$.

*Exp.* IV. To confirm the preceding experiments, I made a new cord as above compofed of

5 threads, 4 of which had born 7 pounds, and broke with 7 pounds and ½; and the fifth had born 6 pounds, and broke with 6 pounds ½; the sum of the strength of these threads was therefore at least 23 pounds; the cord however broke after having for some time sustained a weight of 22 pounds. As I knew by the preceding experiments, and by several others which I do not think it necessary to relate, that the thread which I made use of, had in the weakest places sufficient strength to sustain a weight of 6 pounds, and that it was often strong enough to sustain 9 pound; I thought it right to make my calculations, without examining any more the strength of the thread which I used, and that when I should find that the strength of the cord should be less than that of the sum of the threads, by considering them as not being able each of them to sustain above 6 pounds, that I should not run any risque of being mistaken, since I had never found the strength of these threads less, and that I had commonly found them greater. I therefore again made different cords with the same thread, because we cannot too often repeat the experiments, before we conclude any thing from them.

*Exp.* V. I made a cord with 6 threads; it ought at least to have sustained 36 pounds if the strength had been equal to that of the sum of the threads, and this cord broke with a weight of 31 pounds.

*Exp.* VI. A cord of 10 threads very well twisted, which should at least have sustained 60 pounds, if its force had not been less than that of the sum of the threads, broke being charged with 50 pounds.

*Exp.* VII. Having made a cord by doubling the longest of the two ends, which I had left of the

preceding cord as it was compofed of 10 threads ; we fee that I made a cord of 20 threads, which could not carry lefs than 120 pounds, without being much weaker than the fum of the threads, and lefs than 100 if its ftrength was not diminifhed by the laft twifting. A weight of 80 pounds broke this cord ; its ftrength was therefore diminifhed by the laft twifting.

*Exp.* VIII.   Another cord made of 28 threads, which would at leaft have born 168 pounds, if the twifting had not diminifhed the ftrength of the cord, was broken by a weight of 82 pounds ; I made feveral other experiments, which had the fame fuccefs, and which it would be to no purpofe to relate ; However that it may not be imagined that the cords which I made were too much or too little twifted, and that perhaps the fame thing does not happen to the cords of thread, or hemp, made by the ropemakers, I made trial of thefe laft. Among the various experiments that I have tried upon thefe forts of cords, I fhall content myfelf with relating the 2 following, becaufe all thofe that I made have not fucceeded differently.

*Exp.* IX. I took a fmall hempen cord, very well made by a rope-maker ; it was made of three other fmaller cords, each of which was compofed of two coarfe threads of hemp.   I call thofe threads which are not made of other fmaller cords, but are compofed of divers filaments of hemp or flax.   Having fixed a weight of 50 lb. to the cord juft mentioned, it broke in an inftant ; as it feemed to me that this cord ought to have been ftronger, I fufpended afterwards feveral weights to the longeft end that was left ; it fuftained 72 lb. and broke being charged with 75. To know if the fum of the forces of the three little cords which compofed this, was greater than that

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of this cord, I untwifted it, and having tried the ftrength of thefe little cords by different weights, I found that one bore 27 lb. without breaking, the other 33 lb. and the laft 35 lb. The fum of the ftrength of thefe 3 cords was therefore at leaft equal to that which is required to a weight of 95 lb. yet the cord which they compofed had firft broken at 50 lb. and afterward at 75; its ftrength was therefore much lefs than that of the fum of the threads.

As to the reft it muft be obferved, that if I had fought the ftrength of the 2 threads, which each of the 3 little cords was compofed of; the fum of the forces of thefe two threads, would perhaps have been found lefs than that of the little cord which they compofed; and that by a reafon particular to cords which are made of filaments, fhorter than the cords themfelves. Which is, that each of the filaments cannot exercife its whole ftrength, unlefs the refiftance of the friction which it muft overcome to flip, does not furpafs the ftrength which this filament has to fuftain a weight. Now it often happens, that the threads are not enough twifted, becaufe the filaments of hemp, or flax, which compofe them, cannot flip fo eafily as not to be broken. But when we make a cord, for example, with two or three of thefe threads, the new twifting which is given them, adds to the filaments which compofe them, what they want of friction, and puts them in a ftate of being broken by a lefs force than is neceffary to make them flide, and when each filament fhall be more eafily broken than difengaged from thofe which encompafs it, the ftrength of the cord will always be lefs than the fum of the forces of the threads or filaments which compofe it.

*Exp.*X. Another cord pretty near the same thickness with the preceding, will also serve for a new proof. It suftained a weight of 70 lb. and broke, about the middle by the weight of 72 ; I faftened a weight of 75 lb. to the longeft bit that remained, to fee if the cord had not broken in a place much weaker than the reft ; but it could not suftain the weight of 75 lb. Having fought feparately the ftrength of the 3 little cords of which it was made, the firft bore 24 lb. and broke with 28 ; the fecond bore 28 lb. and broke with 29 ; and the third fuftained 30 lb. and broke with 31. The fum of the ftrength of thefe 3 cords was therefore at leaft equal to 82, and confequently greater than that of the broken cord by a weight of 71 lb.

There is no doubt but that the experiments which I made, would have fucceeded in the fame manner upon thicker cords ; the great number of threads, or of fmall cords, cannot make any alteration. But the experiments would have been much more difficult to execute, and the preceding are fufficient. I fhall however relate one, which I made upon a bit of filk, fuch as is commonly ufed for fewing ; notwithftanding the fmallnefs of thefe forts of cord, we may compare it to the thickeft cables, if we only confider the number of fingle threads which compofe it. The threads of this bit of filk were exceeding fine ; it alfo contained a much greater number of filaments than the bits which I have fpoken of in the *Examination of the filk of fpiders\**. For having feparated it with great attention and papatience, I divided it into 832 fingle threads, whereas I never found but 200 threads in the

\* Page 41 of this volume.

others.

others. If there was any miftake in this calculation, it could be only in making the number of threads lefs than it really was, becaufe it might eafily happen that the extreme finenefs of thefe threads might make me fometimes take two for one; but this number cannot be too great, becaufe I never counted one thread without feparating it well from the others. I had alfo the precaution to cut it after having counted it for fear I fhould make a double work of it.

Thefe 832 threads compofed two different little cords, which being twifted about one another, made the bit of filk; having fucceffively fixed different weights to this bit of filk, I found that it commonly fuftained 5 lb. for fome moments, after which it broke; but it was feldom ftrong enough to bear 5 lb. $\frac{1}{2}$ and in a great number of experiments it was not above once or twice, that 5 lb. $\frac{1}{2}$ did not make it break. Having afterwards examined the ftrength of the threads which compofed this piece of filk, I was convinced by feveral experiments, that the weakeft could fuftain a dram without breaking; and the ftrongeft a dram and $\frac{1}{2}$; we fee therefore, that if thefe threads are much finer than thofe which I fpoke of in the *Examination of the filk of fpiders*, they are alfo much weaker, for thofe fuftained 2 drams $\frac{1}{2}$. Since thefe threads bore at leaft a dram, and the ftrongeft, of which I found a much greater number than of the weakeft, bore 1 dram and $\frac{1}{2}$, it is evident that I am not too favourable to the fum of the forces of the thread, when I take 1 dram, 18 grains for the mean force of each thread. And according to this fuppofition, the fum of the forces of the thread which compofe this piece of filk, was 1040 drams; or dividing this fum by 128 to

reduce

reduce it into pounds; the fum of the forces of the threads was 8 lb. 2 ounces. Now we have feen above, that the filk did not in general fuftain above 5 lb. and but feldom 5 $\frac{1}{2}$; its force was therefore confiderably lefs than the fum of the threads? If we had taken the force of the weakeft threads, which was a dram, for the true force of each thread, the fum of the forces would have been 832 drams, that is, 6 lb. $\frac{1}{2}$ and confequently greater than that of the bit of filk.

We may therefore certainly conclude from all thefe experiments, that the force of a twifted cord is lefs than the fum of the forces of the threads which compofe it. But it is not poffible to determine in what proportion the twifting diminifhes it, becaufe this diminution depends upon a great number of irregularities, each of which may be combined in many different manners.

Thefe experiments teach us at leaft, that when we can employ, in a convenient manner, many little cords, and can ftretch them equally; thefe little cords would be in a ftate of producing a greater effect, or of refifting a greater effort, than a cable compofed of all thefe fmall cords would be.

Laftly, if we cannot decide what the ftrength of a cable is, we may decide between what limits it is inclofed, by feeking what the force of one of the fmall cords is which compofe it; and by examining what is the number of thefe cords; fince we have feen, that the force of the cable is lefs than the fum of the forces of all thefe cords.

IV.

IV. *Remarks on some colours, by M.* de la Hire

The deep purple red only appears bright and shining when it is expofed to a ftrong light ; but when we view it in a moderate light, it appears to us very dark, and approaching to black.

We alfo know, that when we view a luminous or very bright body through a dark and rare one, it appears red to us, as when we look at the fun through a fmoaked glafs ; and we cannot fay, that it is the proper colour of this fmoke which gives it this red, fince this very fmoke being mixed with white, makes a colour very much inclinable to blue, which is far from red.

For the explaining this red colour, we muft have recourfe to what we can imagine of the fenfation of red, which is only a violent fhaking of the *retina*, with a certain modification, which is not found in the violent fhaking of the *retina*, by reflection alone, which only caufes white ; and if the choroides, which, according to my fyftem, receives the impreffions of light, to tranfmit them to the *retina*, is very fenfible, and very thick, it muft happen that the modified light which gives us the fenfation of red, meeting this choroides, is intirely abforbed, and fhakes the *retina* only as if it was a black body. This is alfo what we obferve in fome eyes, which being otherwife very good for feeing the fmalleft objects very diftinctly, fee red only as if it was black, and have no idea of what we call red ; and for other colours they fee them perfectly well.

We know alfo, that when we fee a black body through a white and thin one, it gives us the fen-

* March 28, 1711.

fation

fation of blue, and there can be no doubt of it, since this is the only reason why the fky appears blue to us; for its immenfe depth being intirely deprived of light, can only appear to us, through particles of air, which are enlightened by the fun, and appear white. This is alfo the reafon why the black of fmoke diluted with white, appears blue; for the bodies which appear white, being always a little tranfparent, and being confounded with the black behind them, give a fenfation of blue.

Thefe two explications of red and blue, fhew us why the veins which are feen upon the furface of the fkin, efpecially if it be very white, appear blue to us, altho' they are filled with a very red blood.

For by what I have here above explained, it is evident that the blood which is of a deep red, being inclofed in the veins, is there in fome meafure in obfcurity, and confequently would appear like black, and this black being viewed through the membrane of the vein, and through the white skin, gives us a fenfation of blue; which cannot happen to the reft of the skin which is white, and filled with innumerable particles of blood quite to the cuticle, which muft appear to us of a white inclined to a vermillion, for thefe particles of blood are very much difperfed: but if it fhould happen, by any accident, as by fome blow, that the blood gathers together in a great quantity under the skin in fome place, the part prefently appears bluifh, and we fay it is bruifed.

It is alfo without doubt this blue colour of the veins, which has led anatomifts, who inject wax into the veffels of the body, to fyringe blue wax into the veins and red wax into the

arteries

arteries, to diftinguifh them from the veins, and to fhew a little the different nature of the blood of thefe veffels, for it is much more vivid, fpirituous and florid in the arteries than in the veins.

V. *Reflections on fome new obfervations of F. Feuillée made in the* Weft-Indies, *extracted from a letter written to M.* le Comte de Pontchartrain *from* Lima, Dec. 7, 1709, *by M.* Caffini, *the fon*†.

At the beginning of his voyage he was obliged by contrary winds, to ftop at *Sardinia* and *Malta*, which gave him the opportunity of making feveral aftronomical and phyfical obfervations, the extract of which is related in the *Memoirs of the Academy of* 1708. He alfo made feveral obfervations in the *Mediterranean*, for determining the latitude of the places where he had the convenience of obferving, of which this is an extract.

*Height of the pole at* Golfo di Palma, *in the ifland of* Sardinia.

The height of the pole of this gulf which is between the ifland of *St. Antiocho*, and the main land of *Sardinia*, was obferved to be ——— 38 59 24

*Height of the pole at* Port-Mahon.

The height of the pole at *Port-Mahon*, which is in the ifland of *Minorca*, was obferved to be ——— 39 53 45

† Dec. 17, 1710.

*Height*

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*Height of the pole at* Carthagena.

The height of the pole at *Cartha-gena*, in *Europe*, was obferved in the port to be ——— ——— 37 36 8

*Height of the pole at* Almeria.

The height of the pole at *Almeria*, which is in the kingdom of *Granada*, was obferved to be ——— 36 50 18

F. *Feuillée* afterwards paffed the ftreights of *Gibraltar*, continuing his way toward *America.* He obferved during the courfe of his navigation, that the fea-water diminifhed in its weight, in pro-portion as he approached the line. He made daily obfervations of it in prefence of the officers of the fhip, and does not believe that the mixing of the frefh-water has contributed to this alteration, having paffed the line at a very great diftance from *Africa* and *America*. He obferved alfo during his voyage, the variation of the needle, and the place of his courfe where there is no variation.

The firft place where he arrived in *America*, was *Buenos-Ayres*, where the bad weather would not permit him to make any obfervations of the fatellites of *Jupiter*.

*Obfervations made at* Buenos-Ayres *on the* Rio del Plata, *the* 19*th of* Aug. 1708.

The height of the pole was obferved at *Buenos-Ayres* to be ——— 34 34 44

This obfervation was confirmed by thofe of the 20th and 21ft of *Aug.* which give the fame height to the pole within a few feconds.

### *The* 21*ſt of* Aug.

F. *Feuillée* obſerved at *Buenos-Ayres* the variation of the needle to be 15° 32′ NE

He obſerved alſo, that the needle of the compaſs dipped at the ſouth point, and made an angle with the true horizon of ——————  6° 20′

### *Obſervations made at* Monte Vidio, *for the height of the pole, the* 23*d of* Oct.

F. *Feuillée* obſerved at *Monte Vidio,* which is to the ſouth-eaſt from *Buenos-Ayres,* at the outlet of the *Rio del Plata,* the height of the pole to be 34° 52′ 30″

It was obſerved the 24th and 28th, to be within a few ſeconds the ſame.

### *Obſervations made at the* Conception *in the kingdom of* Chili.

F. *Feuillée* determined the height of the pole at the *Conception,* by a great number of obſervations of the meridian height of the ſun, and fixed ſtars, made in *Jan.* and *Feb.* between which, if we take a mean, we ſhall have for the height of the pole at the *Conception* 36° 44′ 30″

### *Eclipſes of the ſatellites of* Jupiter *for the longitude of the* Conception, *the* 31*ſt. of* Jan. 1709.

At 0h 3′ 23″ in the morning, immerſion of the firſt ſatellite into the ſhadow of *Jupiter.*

5 5 28 at *Paris* by the corrected calculation.

<span style="font-variant: small-caps;">h ′ ″</span>
5 2 5 difference of the meridians be-
tween *Paris* and the *Conception.*

### *The 7th of* Feb.

1 55 36 in the morning immerſion of the
firſt ſatellite into the ſhadow
of *Jupiter.*

6 58 8 at *Paris* by the correƈted calcu-
lation.

5 2 32 difference of the meridians between
*Paris* and the *Conception.*

### *The 9th of* Feb.

11 0 52 at night, the immerſion of the
third ſatellite into the ſhadow
of *Jupiter.*

16 1 50 at *Paris* by the correƈted calcu-
lation.

5 0 58 difference of the meridians be-
tween *Paris* and the *Conception.*

### *The 17th of* Feb.

0 34 4 in the morning immerſion of the
third ſatellite into the ſhadow of
*Jupiter.*

5 31 5 at *Paris* by the correƈted calcu-
lation.

4 57 1 difference of the meridians be-
tween *Paris* and the *Conception.*

### *The 18th of* Feb.

1 45 2 in the morning immerſion of the
ſecond ſatellite in the ſhadow of
*Jupiter.*

At $6^h\ 46^\prime\ 17^{\prime\prime}$ at *Paris* by the corrected calculation.

$5\ 1\ 15$ difference of the meridians between *Paris* and the *Conception.*

Taking the mean between the differences of the meridians determined by the obfervations of the firft fatellite of *Jupiter*, which only differ 19 feconds from one another, we fhall have the difference of the meridians between *Paris* and the *Conception* ———  $5^h\ 2^\prime\ 14^{\prime\prime}$

This difference being brought into degrees, gives the difference of the longitude between *Paris* and the *Conception* of  ——— ———  $75\ 33\ 30$

By which the *Conception* is more wefterly than *Paris.*

*Obfervations of the variation and inclination of the needle, the 27th of* Jan. 1709.

The variation of the needle was obferved at the *Conception* to be  $10^\circ\ 20^\prime$NE

And the inclination of the needle  $6\ 35$

*Obfervations made at* Valparaifo, *upon the coaft of the kingdom of* Chili.

F. *Feuillée* determined by the meridian heights of the fun and fixed ftars, the height of the pole at *Valparaifo* to be  ——— ———  $33^\circ\ 0^\prime\ 11^{\prime\prime}$

*Eclipfe*

*Eclipfe of the firft fatellite of* Jupiter, *for the lon-gitude of* Valparaifo, *the* 11*th of* March, 1709.

At 10$^h$ 34$'$ 13$''$ at night, immerfion of the firft fatellite into the fhadow of *Ju-piter.*

15 32 28 at *Paris* by the correcfted calcu-lation.

4 58 15 difference of the meridians be-tween

*Paris* and *Valparaifo,* which being brought into degrees, gives the diffe-rence of longitude between *Paris* and *Valparaifo* ———— ———— 74$^o$ 33$'$ 45$''$

By which *Valparaifo* is more wefterly than *Paris.*

*Eclipfe of the fun,* March 11, 1709.

The horizon was full of clouds at the rifing of the fun, and they could not obferve it till a little after.

*The* 11*th of* March.

At 6$^h$ 19$'$ 46$''$ in the morning, the fun was eclipfed one digit.

6 27 37 end of the eclipfe.

F. *Feuillée* could only obferve thefe two phafes of the eclipfe ; and he obferves, that in this coun-try the fun is but feldom feen.

This eclipfe having been obferved at *Paris,* and feveral other parts of *Eu-rope,* we have defcribed the parallel of *Valparaifo* in the figure of this eclipfe, and found by the phafe of a digit, the difference of the meridians between *Paris* and *Valparaifo* to be 4$^h$ 55$'$ $''$

And by the end ————— 4 53 50

Thefe

Thefe differences are lefs than thofe which are found by the obfervation of the firft fatellite of *Ju-piter*, to which it is beft to keep ; becaufe of the fimplicity of the elements which are ufed for the comparifon of thefe obfervations.

*Obfervations made at* Lima, *the capital of* Peru.

F. *Feuillée* determined by the meridian height of the fun, the height of the pole at *Lima* to be   ——    12  1  15

Whilft he ftayed at *Lima* he obferved geometrically the height of a mountain which he found elevated 143 toifes, and almoft 5 feet above the horizon. He took at the foot of the mountain, the height of the barometer, which he found to be 27 inches 5 lines, which is 10 lines $\frac{3}{4}$ higher than the 26 inches 6 lines $\frac{1}{4}$, which he found at the top of the mountain.

According to the rule drawn from our obfervations, the difference in the height of the barometer which agrees to the height of 143 toifes, 5 feet above the horizon of the fea, ought to be 12 lines $\frac{8}{9}$, greater by two lines than what F. *Feuillée* has found, which makes him judge that the condenfation and dilatation of the air in *America*, is very different from that which is obferved in *Europe*. We fhall not give a further detail of this obfervation, F. *Feuillée* having a defign of meafuring a fecond time the height of this mountain before his departure.

With regard to the height of the barometer at the fea-fhore, F. *Feuillée* found by the obfervation, which he every day made at land, that it is pretty near the fame as in *Europe*.

During his ftay at *Lima* they felt there feveral earthquakes, and the 7th of *Dec.* in the morning,

ing, the day of the date of his letter, they had two fhocks fo ftrong, that had they lafted a little longer, there would not have been any building that could have refifted them.

VI. *An extra*ct *of feveral obfervations made by* F. Feuillée *in the* Weſt-Indies, *by* M. Caſſini *the fon\**.

Since the obfervations that F. *Feuillée* made in his voyage to the *Weſt-Indies*, which we have related to the royal academy of fciences, he has fent others to M. *le Comte de Pontchartrain*, which he has continued to make in the *South-fea*.

*Obfervations made at* Coquimbo *for the longitude, the 16th of* April, 1710.

At 11$^h$ 57$^\prime$ 36$^{\prime\prime}$ at night, immerfion of the fecond fatellite in the fhadow of *Jupiter*.

16 51 32 at *Paris* by the corrected calculation.

4 53 56 difference of the meridians between *Paris* and *Coquimbo*.

*The* 22d *of* April, 1710.

At 0 6 25 in the morning immerfion of the firft fatellite into the fhadow of *Jupiter*.

5 1 8 at *Paris* by the corrected calculation.

4 54 43 difference of the meridians between *Paris* and *Coquimbo*,

Which being brought into degrees, gives the difference of the longitude between *Paris* and *Coquimbo* of　　73 40 45

\* July 8, 1711.

*Obfervations made at* Coquimbo *for the height of the pole, the* 17th *of* April, 1710.

F. *Feuillée* obferved at *Coquimbo* the meridian height of the upper edge of the fun, to be ———— $49 \ 51 \ 50$

He continued obferving it the following days till the 28th of the fame month.

According to thefe obfervations, having regard to the refraction, the femi-diameter of the fun, and its parallax, we fhall find the height of the pole at *Coquimbo* to be ———— $29 \ 54 \ 30$

*Obfervations of the variation and inclination of the needle at* Coquimbo.

The variation of the needle was obferved at *Coquimbo* to be ———— $8 \ 32$ NE

And the inclination of the needle $5 \ 25$

*Obfervations for the height of the pole at* Arica, *the* 20th *of* May.

F. *Feuille* obferved at *Arica* the meridian height of the upper edge of the fun to be ———— $51 \ 49 \ 45$

And the 21ft of *May* ———— $51 \ 37 \ 34$

By thefe obfervations we find the height of the fouth-pole at *Arica* to be $18 \ 28 \ 0$

*Obfervations made at* Ylo *for the longitude, the* 24th *of* July, 1710.

At $9 \ 24 \ 57$ at night, emerfion of the firft fatellite out of the fhadow of *Jupiter.*

At 14$^{h}$ 19$'$ 11$''$ at *Paris* by the corrected calcu-
lation.

4 54 14 difference of the meridians be-
tween *Paris* and *Ylo*,

Which being reduced into degrees
gives the difference of longitude be-
tween *Paris* and *Ylo* of ———— 73$^{o}$ 33$'$ 30$''$

By which *Ylo* is more wefterly than *Paris*.

*Obfervations for the height of the pole at* Ylo, *the*
*5th of* June.

F. *Feuillée*, obferved at *Ylo* the me-
ridian height of the upper edge of 50$^{o}$ 5$'$ 30$''$
the fun to be — — — — —

He continued obferving it the following days
'till the 25th of *July*.

According to thefe obfervations we
find the height of the pole at *Ylo* to be 17$^{o}$ 36$'$ 30$''$

*Obfervations of the variation and inclination of the*
*needle at* Ylo.

The variation of the needle was
obferved at *Ylo* to be ———— 6$^{o}$ 38$'$ 0$''$ NE
and the inclination of the needle 3 45 0

Thefe obfervations, added to thofe which we
have already related, determine more particularly
the fituation of the weft coaft of *South America*,
which 'till now has been but little known.

VII. *Experiments on the thermometer, by*
M. de la Hire *the fon*\*.

A canon of *Chartres* one of my friends, and
an excellent naturalift, wrote to me in *February*
1709, that having feen the papers that *M. Nuguet*

\* June 13th, 1711.

had

had publifhed, where he gave the conftruction of a new thermometer, which as he pretended was free from the faults of others, and that having alfo feen the reflections that I have given upon this thermometer, printed in the *memoirs of the Academy* of 1706, he had a mind to examine whether this new thought, fo oppofite to that of M. *Amontons*, was founded upon any certain principle.

He took for his experiments a thermometer which he had caufed to be made at *Chartres*, from one of thofe of M. *Amontons*, the ball of which was 13 lines outfide diameter, and the tube 3 feet 2 inches long, to $\frac{2}{3}$ of a line of infide diameter.

The 7th of *December* 1708, he put this thermometer into water, which he let freeze, without taking notice at what height the fpirit of wine was in the tube, and when the water was perfectly frozen, the fpirit of wine was at 11 inches 7 lines above the ball ; he afterwards melted the ice from the thermometer, by holding it to the fire, and believed that the fpirit of wine could not defcend lower in this thermometer ; not imagining that there could be a greater cold than that of water very hard frozen.

He afterwards expofed this thermometer to the cold of the following days, and found that the fpirit of wine fell in the tube to 1 inch lower than it had fallen being in water very hard frozen, that is, 1 inch below the 11 inches 7 lines ; he believed that the caufe of this effect proceeded from this, that the water was not perfectly frozen in its whole mafs, for this reafon he repeated the preceding experiment.

*January* the 8th 1709, the cold being very great, at 8 in the morning he put the fame ther-

mometer

mometer, of which the spirit of wine was at 9 inches 8 lines above the ball, into water which was frozen in a very little time, and examining very attentively what should happen to the spirit of wine in the tube, he saw that in less than half a quarter of an hour, the spirit of wine rose 2 inches $\frac{1}{2}$ a line above the 9 inches 8 lines. An hour after, it was also risen $\frac{1}{4}$ a line more, at noon another $\frac{1}{2}$ line, and at half an hour after 9 at night, it was risen 2 inches 4 lines $\frac{1}{3}$ above the nine inches 8 lines. He left this thermometer all night to the air and in the ice, the cold being very great, so that the ice swelled and raised itself above the edge of the vessel, and the next day in the morning about the sun's rising, he found that the spirit of wine was risen in the tube more than 2 feet above the ball, and that it was interrupted with many bubbles of air very much extended. He here finished his experiment, and wanting to disengage the thermometer from the ice, he broke it.

This experiment did nothing but puzzle my friend, for he saw the contrary happen to what he expected, and to what had happened in the first experiment, since instead of seeing the spirit of wine sunk in the tube after the thermometer was in the ice, he saw it rise continually to the height of above 2 feet, and interrupted with many bubbles of air, which made him think that the ice had perhaps made the spirit of wine ferment, from whence he concluded that *M. Nuguet*, who made use of the cold of water in which he put ice to construct his thermometer, would have found it difficult always to meet with the same degree of cold.

I answered my friend some days after, and acquainted him that the experiment had surprized me,

me, and that I did not believe the great cold had caufed a fermentation in the fpirit of wine, but I was perfuaded that what made it rife 2 inches $\frac{1}{2}$ a line, during the firft half quarter of an hour, that the thermometer was in the water, was, that when he had put it in, the fpirit of wine, which was ex remely condenfed by the great cold, becaufe it was expofed to the air, and the water was not, had in a manner thawed, that is, the particles of cold which were within, had gone out, and were joined to thofe of the water, which was not fo cold as the fpirit of wine, fince it was not frozen, as it happens to fruits that are frozen, when they are put into water that is near freezing.

As to the elevation of the other 4 lines, which the fpirit of wine rofe in the tube, during the reft of the day, it might very well come from this, that the water being afterwards frozen very hard, it had according to cuftom increafed its bulk, and made an effort againft the fides of the veffel, and againft the ball of the thermometer, of which it had by this means diminifhed the bulk.

After having given him a reafon for the elevation during the day, it remained to explain how the fpirit of wine had been able to rife to fo great a height in the night, and from whence the bubbles of air came, which were found mixed with it.

I could not find a more probable explication, not knowing how to admit the fermentation, unlefs it was that the water having continued to freeze more and more, had confiderably increafed its bulk, fo that the fides of the veffel not being able to give way, the whole effort was united againft the ball of the thermometer, which

U 2

not having ftrength enough to refift it, for want perhaps of not having been perfectly fpherical, or of the fame thicknefs in its whole extent, was broken; and then the preffure of the ice, joined with many bubbles of air which efcaped from it one after another, and were entered into the fpirit of wine, had obliged it to rife in the tube to fo great an height, being interrupted with bubbles of air.

Thefe were the reafons I then made ufe of, to explain to my friend the experiments he had communicated to me; however, as I was afraid of being miftaken in my conjectures, I refolved the next winter to make his experiment; but having only one cold day in 1710, I could not execute what I had projected, and was obliged to defer it till this year 1711, in which we have had fome days cold enough do it.

The 2d of *February* at 11 in the morning the thermometer, which always is in the eaft tower of the obfervatory, and with which we make our experiments, being at 24 ½ parts, I expofed upon the window of this tower, which looks to the north, an iron mortar full of water, in the middle of which I had fufpended the ball of a thermometer which I had carried into the caves of the obfervatory, to have a fixed point from whence I might meafure the fallings and rifings of the fpirit of wine in the tube, becaufe we know that the temperature of the air never changes in thefe caves, and that it is taken for a mean ftate.

After the thermometer had been a quarter of an hour in the water, upon which there began to be formed a cruft of ice, I found that the fpirit of wine was fallen in the tube 3 inches 1 line below the mean ftate, I continued ob-

ferving

ferving it every quarter of an hour, 'till 5 in the afternoon, to fee the alterations which might happen to it, but I did not obferve any during the whole time, and the fpirit of wine always remained at the fame place, 'tho the water was continually more and more frozen, and then feemed to me entirely fo through the mafs; the thermometer with which we made our experiments had not changed fince the morning, and had always remained at 24 $\frac{1}{2}$ parts.

I left my thermometer in experiment, in the fame place, all night, between the fecond and third, and the 3d at 8 in the morning, our common thermometer being at 21 parts, that is at 3 $\frac{1}{2}$ parts lower than the preceding day, I found that which was in experiment in the ice, the furface of which was fplit in many places, probably by the ftrength of the cold, was funk 5 inches 10 lines $\frac{2}{3}$ below the mean ftate, and confequently 2 inches 9 lines $\frac{2}{3}$ lower than the preceding day.

The fame day at 11 in the morning, I put the thermometer, that was in experiment, near our common thermometer, which was at 24 $\frac{1}{4}$ parts, as the preceding day; and after having left it there fome time, I found that which was in experiment rifen again to 13 lines above what it was at 8 in the morning; ever fince I have always left them together to compare them.

The 4th, at three quarters after feven in the morning, the common thermometer was 23 $\frac{1}{2}$ parts, and that which was in experiment was 3 inches 8 lines below the mean ftate.

The 5th, at half an hour after feven in the morning, the common thermometer was at 26 $\frac{1}{2}$ parts, and that which was in experiment was 3 inches 8 lines below the mean ftate.

The

The 6th, at half an hour after feven in the morning, the common thermometer was at 25 ½ parts, and that which was in experiment was 3 inches 11 lines below the mean ftate.

The 7th, at half an hour after feven in the morning, the common thermometer was at 23 ½ parts, and that in experiment was 4 inches 3 lines below the mean ftate.

The 8th, at three quarters after feven in the morning, the common thermometer was at 23 parts, and that in experiment, was at 4 inches 3 lines ½ below the mean ftate.

The 9th, at three quarters after feven in the morning, the common thermometer was at 30 parts, and that which was in experiment was at 3 inches 1 line ½ below the mean ftate.

The 10th, at noon, the common thermometer was at 40 parts and ½, and that which was in experiment was at 1 inch 9 lines ¼ below the mean ftate, and was plunged in the water which was come from the ice in the mortar, and was melted during the preceding night.

In comparing thefe experiments together, we fee that the thermometer which was in the ice has pretty well followed that which was not, when the cold became greater ; but when it diminifhed, that which was in the ice did not follow it fo well, and it could not rife fo eafily as the other, becaufe of the cold of the ice, which furrounded it.

I was furprized the 2d of *February*, when I put my thermometer into experiment, to fee that a quarter of an hour after it was put there, during which time it had funk, becaufe before that time it had been in a place lefs cold than the open air, it did not change for fix hours, altho' the water was entirely frozen, but the ex-

periments

periments which I made afterwards fhewed me the reafon of it, by letting me fee that the degree of cold of the air, which did not change in thefe fix hours, was greater than was neceffary to freeze the water, and fo the thermometer which was within, could not receive any impreffion from it ; yet it happened quite the contrary to that of my friend, for during the firft quarter of an hour, it rofe inftead of falling, from whence it muft be concluded, that the water was lefs cold than the fpirit of wine of the thermometer, which was ex-pofed to the air, and it really was fo, for it froze in a very little time ; but it ought to have funk afterwards, fince the cold paffed through the ice, which did not happen, by the reafons which are related at the beginning of this memoir.

As to the great height where he found the fpirit of wine, interrupted by great bubbles of air in the tube, the next morning, it does not appear to me that this effect could proceed from any other caufe than the breaking of the ball, as it is here above obferved, fince the temperature of the air had hard-ly changed from that day to the next.

It does not feem to me, that it can be faid, that the cold of ice is always the fame cold, fince we have feen by the experiments juft mentioned, that the greater or lefs cold of the air, is very fud-denly felt upon the ball of the thermometer which is inclofed in the ice, and if the fpirit of wine is fufceptible of alteraion through the folid ice, what will it not be when it fhall be broken, or put in-to water.

VIII.

VIII. *New experiments on the dilatation of the air, made by* M. Scheuchzer, *upon the mountains of* Switzerland, *with reflections thereon, by* M. *Maraldi* *; *tranflated by* Mr. Chambers.

M. *Scheuchzer* having fent feveral experiments on the dilatation of the air, made on the mountains of *Switzerland*, in *September* 1710, to the *Abbé Bignon*, who defired M. *Scheuchzer* to make fome further obfervations, to learn whether the dilatation of the air at great heights follow the fame proportion, as near the furface of the earth.

Accordingly with a tube 33 inches long, and 2 lines in diameter, he obferved the height of the mercury *in vacuo*, at feven different ftations, making the common obfervations at each ftation, of the dilatation of the air, by leaving firft 3 inches of natural air in the tube, then 6 inches, then 9, and fo fucceffively to 30. And he meafured the height at which the mercury ftood in the tube, after the dilatation, as well as the fpace poffeffed by the dilated air, after the invertion in the loweft of thefe ftations, the mercury ftood *in vacuo* at 26 inches, 7 lines ½, and in the higheft at 21 inches 6 lines, fo that the difference between the height of mercury *in vacuo*, was 5 inches; now to find whether the common rule of the dilatation of the air among us, be conformable to the obfervations of M. *Scheuchzer*, I calculated the fpace which the air was to poffefs in the tube after dilatation, according to this rule, and compared them with the obfervations themfelves.

* March 27, 1711.

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By

By this comparison it appears that the *calculus*, only agrees with observation in the dilatations answering to the 3 first inches of natural air, in the rest the dilatation observed is less than that of the rule, as far as the 18th inch of natural air, where they agree with each other within one or two lines; but from the 18th to the 30th inch of natural air, the dilatation observed is always greater than that calculated, contrary to what obtains in the first 18 inches, the greatest excess of the *calculus* above observation was at the 9th and 10 inch, and was at 8 or 9 lines, and the greatest deficiency of the *calculus* with regard to observation was 10 or 11 lines, at the 24th and 27th inch of natural air:—Which shew that at great heights the air does not dilate at the same rate as it does near the surface of the earth, and consequently that the rule is not general for the whole compass of the air in any climate.

In a former memoir for the year 1709, we have also shewn that this rule does not hold even in air, much at the same height, but in climates very different from our own, as that of *Mallacca* in the *East-Indies*, here by other observations like those of M. *Scheuchzer's* three inches of natural air, after their dilatation possess'd a space of 7 inches 5 lines in the tube, whereas by the rule it should have possess'd 9 inches 6 lines ½, the difference therefore between the observation and rule is 2 inches 1 line.————So 6 inches of natural air after dilatation possess'd 10 inches 9 lines; whereas by the rule the dilatation should have been 13 inches 3 lines, the difference is 2 inches 5 lines, by which the dilatation of the rule comes short of that found by observation, and the like is found in several observations made at

*Malacca,* and calculated in the memoir above mentioned, which shews that in the air of *Malacca* the dilatation is much greater than in ours, and even than what is found in M. *Scheuchzer's* observations.

There is some conformity however between the observations of *Malacca* and those of *Zurick* ; at *Malacca,* where the dilatation of the air is different from that of *Paris,* the variation of the barometer is less than at *Paris,* and the same holds with the observations of *Zurick,* where the dilatation of the air is different from what it is at *Paris,* and the variation of the barometer less than what is found either at *Genoa* or at *Paris* ; 'tis true at *Malacca* the dilatation is different from what it is at *Zurick,* the dilatation observed at the former place being always less than what arises from the *Calculus,* whereas by M. *Scheuchzer's* observations, the dilatation observed proves less than the calculated one to a certain term, after which it is greater——————'Tis observable that at the lowest place where M. *Scheuchzer* made his observations, the dilatation of the air proceeds differently from what it does at *Paris,* though the difference in the height of the mercury, at those two places, be only 2 inches, and yet the dilatation proceeds alike at the several stations, observed by M. *Scheuchzer,* tho' the difference between the highest aud lowest place answered to above 5 inches of mercury, whence it may be inferr'd, that near the surface of the earth, the air varies even in the same climate, but that 'tis more uniform at a great height therefrom.

M. *Scheuchzer* made some experiments in an iron mine, where the air was very hot, on account of the fire kept therein to melt the oar, and found

the height of the mercury in *vacuo*, as well as the dilatation of the air, the fame as they were out of the mine in the open air, which agrees with the experiments related in the memoirs of the year 1709, whereby it appears that a great heat, as that of boiling water, makes no fenfible altera- tion in the dilatation of the air.

IX. *Obfervations of fome eclipfes of the planets and fixed ftars by the moon, made in feveral places, compared together to determine the differences of the meridians, by M. Caffini the fon\*.*

The obfervations of the eclipfes of the ftars by the moon, made in feveral places, being very proper for determining the geographical longitudes of thofe places ; we have thought it neceffary to compare many of thofe which have been hitherto made, to draw this advantage from them.

Among thefe obfervations there are feveral which are related in the *Journaux des Scavans*, in the *Philofophical Tranfactions of the Royal Society of London*, and in the *Leipfick Acts*, from which we have extracted thofe which have been made at the fame time in feveral places.

*Obfervations of the eclipfe of the* Pleiades *by the* moon, *made at* Paris, *and at* Dantzick, *the* 23d of Aug. 1701.

At 13ʰ 31′ 23″ at *Paris* the bright ftar of the
Pleiades of the thir dmagnitude called *Alcyone* by *Riccioli*, en-
ters into the bright part of the moon.

\* Jan. 24, 1711.

<div style="text-align:center">h    ′    ″</div>

15  0  0 at *Dantzick*, *Alcyone* enters into the bright part of the moon.

16  6  55 at *Dantzick*, emerfion from the dark part.

To find by the means of this obfervation the difference of the meridians between *Paris* and *Dantzick*; they firft determined the paffage of this ftar over the meridian which happened at *Paris* at  ————    17  15  34

    Its right afcenfion   ————    52° 4 30

    And its declination   ————    23° 3 41

They have alfo calculated the right afcenfion and the declination of the moon fome hours before or after its conjunction, its diameter, and horizontal parallax.

By this means they have placed in a figure which reprefents the projection of the earth in the orb of the moon, the parallels of *Paris* and *Dantzick*, and the line that the moon defcribed in paffing by this projection.

According to this figure, the immerfion of this ftar ought to have happened at *Dantzick* at  ————    13  56  0

  Which gives the difference of the meridians between *Paris* and this city    1  4  0

By which *Dantzick* is more eafterly, becaufe the hour obferved exceeds that which is marked upon the path of the moon prepared for the meridian of *Paris*.

<div style="text-align:center">*The fame day*</div>

<div style="text-align:center">h    ′    ″</div>

12  55  25 at *Paris*, *Merope* enters into the bright part of the moon.

<div style="text-align:right">14</div>

14 24 30 at *Dantzick* immerſion of *Me-rope* into the bright part of the moon.

15 15 20 at *Dantzick* emerſion out of the dark part.

The paſſage of this ſtar over the meridian happened at

its right aſcenſion was

and its northern declination

| h | ' | " |
|---|---|---|
| 17 | 14 | 26 |
| 51° 47 | 25 |
| 22° 51 | 55 |

By the immerſion of this ſtar into the bright part of the moon, we ſhall have the difference of the meridians between *Paris* and *Dantzick* to be    1ʰ 4 34

*The ſame Day,*

12 21 20 at *Paris* immerſion of *Electra,* into the bright part of the moon.

13 40 0 at *Dantzick,* immerſion of *Electra* into the bright part of the moon.

14 50 0 at *Dantzick* emerſion out of the dark part.

The paſſage of this ſtar over the meridian happened at

its right aſcenſion was

and its northern declination

| h | ' | " |
|---|---|---|
| 17 | 12 | 56 |
| 51° 25 | 6 |
| 22° 51 | 55 |

By the immerſion of this ſtar into the bright part of the moon, we ſhall have the difference of the meridians between *Paris* and *Dantzick.*    1ʰ 2 55

*Eclipſe* of Mars *by the Moon, obſerved at* Dantzick, Oxford, *and* Greenwich, *the* 31ſt *of* Auguſt, 1676.

13 35 42 at *Dantzick* immerſion of *Mars* into the bright part of the moon.

14 46 29 emerſion out of the dark part.

$\overset{h}{12} \overset{\prime}{10} \overset{\prime\prime}{42}$ at *Oxford* immerfion of *Mars*, into the bright part of the moon.

13 10 41 emerfion out of the dark part.

12 14 58 at *Greenwich* immerfion of *Mars* into the bright part of the moon.

13 10 51 emerfion out of the dark part. There is an error in the hour of this obfervation, for inftead of    —    $\overset{h}{13} \overset{\prime}{10} \overset{\prime\prime}{51}$

We muft read    ———    13 15 51

Which we fee by comparing the hour obferved with the correcled hour, the difference between thefe hours which in all the obfervations is     0   4   55

In this being only    —    0   0   5

Having calculated the right afcenfion, and the declination of *Mars*, and of the moon, at the time of its conjunction, and fome hours before or after, they defcribed the path of the moon, in a figure which reprefents the projection of the earth in the orb of the moon, and they have there determined the north pole, and the parallels of *Dantzick*, *Oxford*, and *Greenwich*.

By the immerfion of *Mars* obferved at *Dantzick*, and at *Greenwich*, they find the difference of the meridians between thefe places    —    $\overset{h}{1} \overset{\prime}{15} \overset{\prime\prime}{12}$

And by the emerfion    ———    1 15 0

By the immerfion of *Mars*, obferved at *Dantzick* and *Oxford*, they find the difference of the meridians between thefe two cities    ———    1 20 23

And by the emerfion    ———    1 20 20

This obfervation is related in the *Journal des Scavans* of the 18th of *Jan.* 1677, where it is remarked that Dr. *Halley* having carefully con-

fidered

fidered the parallaxes of the moon in the obferva-
tions of this eclipfe made at *Oxford*, at *Dantzick*,
and at *Greenwich*, he found by the immerfion of
*Mars*

| | h | ' | '' |
|---|---|---|---|
| The difference of the meridians be-tween *Greenwich* and *Dantzick* to be | 1 | 14 | 50 |
| And between *Greenwich* and *Oxford* | | 4 | 59 |

And by the emerfion of the fame
planet the firft of thefe differences is
found to be ———— 1 14 41

And the laft ———— 4 59

Thefe differences of the meridians that Dr. *Hal-
ley* has probably found by the old method, which
is by calculating the parallax of the moon at feve-
ral heights, agree within a few feconds with thofe
that I have found by the method of the projection
of the earth in the orb of the moon.

Suppofing the difference of the me-
ridians between the obfervatory of *Pa-
ris* and that of *Greenwich* to be      9 10 (h)

As we have determined by the ob-
fervations of the *Satellites of Jupiter*,
we fhall have the difference of the me-
ridians between *Paris* and *Dantzick*    1 5 55

A little greater than that which is determined
by the obfervation of the *Pleiades* made at *Paris*,
and at *Dantzick*.

*Eclipfe of* Jupiter *by the moon obferved at* Paris,
London, Greenwich, Nuremberg, Leipfick,
*and* Avignon, April 10, 1686.

  h   '   ''
  9 40 21 at *Paris Jupiter* touches the moon.
  9 41 20 he quite difappeared in the inequal-
            lities of the moon's limb.
10 30  2 the preceding fatellite emerges.

h ' "

10 40 24 the firſt limb of *Jupiter* begins to
emerge out of the dark ſide.

10 40 56 the centre of *Jupiter* emerges out of
the moon.

10 41 36 *Jupiter* is intirely emerged.

10 42 49 the neareſt ſatellite to *Jupiter* emer-
ges it, is $\frac{3.5}{6.0}$ diſtant from *Jupiter*.

10 45 1 the next ſatellite emerges, it is di-
ſtant from *Jupiter* a little leſs than
two of his diameters.

10 50 40 the laſt ſatellite emerges.

9 33 0 at *London*, immerſion of the centre
doubtful, becauſe the limb of the
moon was not well terminated.

10 30 0 beginning of the emerſion.

10 31 20 *Jupiter* is intirely emerged.

9 32 30 at *Greenwich* the limb of *Jupiter*
touches the moon.

9 33 42 *Jupiter* intirely hid.

10 30 30 A little part of *Jupiter* emerged.

10 31 36 *Jupiter* intirely emerged.

10 19 56 at *Nuremberg* by M. *Zimmermam*
*Jupiter* touches.

10 20 47 *Jupiter* intirely hid.

11 22 51 *Jupiter* intirely emerged.

11 25 43 the ſatellite which is in the middle of
the three emerges.

11 31 6 the third ſatellite emerges.

10 20 50 at *Nuremberg* by M. *Wurtzelburg*
*Jupiter* touches.

10 22 0 *Jupiter* half eclipſed.

10 22 30 *Jupiter* wholly hid.

11 19 40 *Jupiter* began to emerge.

11 21 20 *Jupiter* wholly emerged.

At  10  30  33  at *Leipſick*, the limb of *Jupiter*
touches the moon.

    31  4 immerſion of the centre.

    31  33 *Jupiter* intirely hid.

   11  35  0 *Jupiter* entirely emerged. [*There
is here an error of the preſs.*]

   11  7  9 at *Dantzick*, the limb of *Jupiter*
touches the moon.

    7  54 immerſion of the centre.

    8  39 *Jupiter* intirely hid.

   11  49  15 beginning of the emerſion.

    50  0 emerſion from the centre.

   11  50  45 total emerſion of *Jupiter*.

    9  42  13 at *Avignon*, immerſion of the
centre of *Jupiter*.

   10  45  26 emerſion of the centre.

By the firſt obſervation made at *Paris*,
and at *Greenwich*, when *Jupiter* touched
the moon, they find the difference of the
meridians between theſe two places   9 20
By the total immerſion ————  9 33
When a part of *Jupiter* was emerged 9 20
When *Jupiter* was entirely immerged 9 34
 By the obſervation made at *Paris*, and
at *London*, they find the difference of the
meridians between theſe two cities, when
*Jupiter* began to emerge ———— 9 50
When he was intirely emerged  10  0
 By the obſervation made at *Nuremberg*
by M. *Wurtzelbaurg*, they find the diffe-
rence of the meridian between *Paris* and
that town when *Jupiter* touched the moon 35 30
By the central immerſion ———— 35  0
When *Jupiter* was intirely hid  34 45

|                                                    |   | ′ | ″ |
|----------------------------------------------------|---|---|---|
| When *Jupiter* began to emerge                     |   | 34 | 55 |
| When he was entirely emerged                       |   | 35 | 0 |

M. *Zimmerman*'s obfervation gives a difference of fome feconds which probably proceeds from the manner of regulating the clock.

By the obfervation made at *Leipfick*, they find the difference of the meridians between *Paris* and that town, when *Jupiter* touched the moon ——— 40 10

  By the immerfion of the centre 40 0

  When *Jupiter* was intirely hid. 39 50

By the obfervation made at *Dantzick*, we find the difference of the meridians between *Paris* and that town, when the limb of *Jupiter* touched the moon ——————

|     | h |   |   |
|-----|---|---|---|
| town, when the limb of *Jupiter* touched the moon | 1 | 5 | 4 |

  By the immerfion of the centre of *Jupiter* ——— ——— 1 4 44

  When *Jupiter* was intirely hid 1 4 39

  When *Jupiter begun* to go out of the moon ——— ——— 1 4 55

  By the emerfion of the centre 1 4 40

  By the total emerfion ——·—— 1 4 15

As for the obfervation at *Avignon*, it would be ufelefs to compare it with that which has been made at *Paris*, there being probably fome error in the time marked by the clock, fince the difference between the hour of the immerfion from the centre of *Jupiter* into the moon; and that which was obferved at *Paris*, is only two minutes.

Taking

Taking a mean between the differences of the meridians which result from these observations, we shall have the difference of the meridians between *Paris* and *London* ———— 9h 55′ to the west.

Between *Paris* and the observatory at *Greenwich* ———— 9 25 ————

Between *Paris* and *Nuremberg* —— —— 35 2 to the east.

Between *Paris* and *Leipsick* —— —— 40 0 ————

Between *Paris* and *Dantzick* ———— 1 4 43 ————

*Eclipse of* Jupiter *by the moon, observed at* Avignon, London, *and* Totteridge, May *the 7th,* 1686.

15h 37′ 23″ at *Avignon*, immersion of the centre of *Jupiter* into the moon.

16 28 24 emersion of the centre.

15 3 30 at *Totteridge*, beginning of the immersion of *Jupiter* into the moon.

The latitude of *Totteridge* is 51° 39′

And according to Dr. *Halley*, this place is 25 seconds to the west of *London*, and is 9 miles distant from it.

15h 49′ 0″ at *London*, total emersion of *Jupiter*.

Al-

Although the obfervation at *Avignon* was made when the centre of *Jupiter* entered into the moon, and when it went out, we may however compare it with thofe which were made at *Totteridge* and *London*, at the beginning of the immerfion, and at the end of the emerfion, by the means of the diameter of *Jupiter* which was then ———————  $\phantom{0}$ ,  "

|   | ′ | ″ |
|---|---|---|
| ...was then | o | 50 |

By the firft obfervation we find the difference of the meridians between *Avignon* and *Totteridge* ————— . 20 20

By the emerfion of *Jupiter* obferved at *London*, we have the difference of the meridians between *Avignon* and *London* 19 20

If we fubftract from the difference of the meridians that we have juft found between *Avignon* and *Totteridge*, 25 feconds, by which *Totteridge* is more wefterly than *London*, we fhall have the difference of the meridian between *Avignon* and *London* ——— ——— ——— 19 55

Taking a mean between the differences which refult from thefe obfervations, we fhall have the difference of the meridians between *Avignon* and *London* — 19 40

From which if we fubftract the difference between *London* and *Paris*, which we have found by the obfervations of the fatellites of *Jupiter*, to be — 9 40

We

We fhall have the difference of the meridians between *Paris* and *Avignon,* by which *Avignon* is more to the eaft     10   0

This difference agrees with, that which we have found by the triangles of the meridian   ————  ————  10  7

A

# A
# TABLE

### OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCXII.

## In the HISTORY.

## In the MEMOIRS.

II. *A comparison of the observations made at* Zurick, *on the rain, and on the barometer, with the foregoing, during the same year.*

III. *Of the flux and reflux of the sea, by M.* Cassini *the son.*

IV *Reflections on the observations of the barometer, taken from a letter written from* Upsal *in* Sweden, *by M.* Vallerius, *director of several mines of copper in those parts, by M.* de la Hire *the son.*

V. *A continuation of the observations on the bezoar, by M.* Geoffrai, *jun.*

VI. *A machine to disengage the horses absolutely and at once from a coach, when they are head-strong and run away, by M.* de la Hire *the son.*

VII. *A comparison of the observations of the eclipse of the moon,* Jan. 23, 1723, *in the evening, made at* Nuremberg, *by* J. P. Wurselbaur; *and at* Paris, *at the royal observatory, by Mess.* de la Hire.

# AN
# ABRIDGMENT
## OF THE

PHILOSOPHICAL DISCOVERIES and OB-
SERVATIONS in the HISTORY of the
ROYAL ACADEMY of SCIENCES at
*Paris*, for the Year 1712.

## I. *On Bees.*

THO' the reputation of bees is very ancient,
and well eftablifhed, yet they have not
been thought fo wonderful as they really are ;
and we may fay of them, as we fometimes fay of
perfons of merit, that they improve upon being
acquainted with them. M. *Maraldi*, who has
obferved them for many years, with attention,
and diligence, has given a very advantageous and
circumftantial teftimony concerning them ; which
we fhall reduce to the points of greateft impor-
tance, and the moft eafy to be underftood.

The bee gathers both wax and honey from
flowers, but not with the fame organs. As the
honey is a liquid fubftance, which exudes from
the flowers by tranfpiration, the bee fucks it with
a tube from the bottom of the empalements, and
attacks only thofe where the empalement is not
too deep for the length of its tube, when it is at
full ftretch ; for it is bent double when the bee is
not gathering honey. The liquor fucked up by
this tube, is drawn into a little bladder, tranfpa-
rent enough to fhew the colour quite thro'. Part of
it ferves for the nourifhment of the animal, and is

diftri-

diftributed into its veffels.   We fhall fay what be-
comes of the other in its proper place.   As for
the wax, which is the duft of the fummits of the
flowers, the bees take it with their fore-feet, and
put it into a little cavity, which they have in their
hinder-feet.   They often fqueeze and prefs it with
their feet, not only that they may have room for
more, but alfo to reduce it into a fitter form for
being carried away.   Sometimes they roll them-
felves upon the flowers, when they are wet, to
carry off little particles of wax, which will ftick
to the hairs that cover their whole body, and fo
load themfelves on all fides.

When the bee is returned to the hive with its
harveft, it either unloads itfelf in an inftant ; or
if it is not able, is fure to be affifted by others.

The defign of gathering the wax, is to form
that wonderful ftructure called a comb : it is for
this, that the bees have always been admired ; and
are ftill more admirable than has been imagined.
The hexagonal figure which they give to the cells
of their comb, would be worthy of the beft geo-
metricians, who know that any number of thefe
figures fills a fpace without leaving any void ; and
that this figure alfo, which has that in common
with the fquare and equilateral triangle, has cer-
tainly the advantage of containing a greater fpace
in the fame circumference.   And befides, of all
the geometrical ways of working, they have
chofen that which is at the fame time the moft
fimple, and the moft convenient for them ; and
confequently they have made a very ingenious
choice.

Though there feems to be nothing in a hive,
but a continual and irregular agitation of feveral
thoufands of infects, flying about at random, yet
there is in reality a great order ; but it muft be

ftudied

ftudied with care. The labours are diftributed as among the beavers. Some bees bring wax between their two jaws, and perhaps they pour out fome liquor upon it, which dilutes and foftens it; fometimes the fame bees raife from this wax the little walls of the hexagonal cells; fometimes others have this office; but, in fhort, thofe which raife the cells are not thofe which polifh the work; they are fucceeded by others which are commiffioned to make the angles more exact, to fmooth and polifh the furface, and to give the laft hand to every thing. And as this cannot be done without cutting off fome particles of wax, and as the bees are extremly frugal, fome take care to carry away thefe particles, which we may be fure will not be loft. M. *Maraldi* has obferved, that the bees which raife the walls, work lefs time than thofe which polifh them, as if the labour of polifhing was the leaft fatiguing.

Their diligence is extreme. A comb of a foot in length, and 6 inches in breadth, containing near 4000 cells, is finifhed in a day. For this, however they muft have all circumftances favourable.

They faften one comb to the top of the hive, from whence they work downwards, provided the top is not a moveable lid; for if it is, they will perceive it, and faften their comb elfewhere. It is not properly wax that they faften it withal; they are very fparing of that, and make ufe of a very coarfe glue.

As the combs are planes perpendicular to the bafe of the hive, which I fuppofe to be circular, if there was one of them, of which the bottom was a diameter, or an intire chord of this bafe, it would cut the hive into two parts, which could have no communication together. The bees fore-

fee this inconvenience, by not making their combs of fo great an extent, and leaving between two neighbouring combs, which are almoft in the fame plane, a fpace by which two bees may pafs a-breaft. They alfo leave fome openings in each comb, that they may not be obliged to go fo far about. Here is a city built with a great deal of skill.

The cells of the combs are intended for two ufes. 1. They are their magazines; they referve in them the honey, which is to be their nourifh-ment in winter. For of that which they take upon the flowers, and which enters into the ve-ficle that we have fpoken of, there is but a fmall part which ferves for their actual nourifhment, they throw out the reft when they return to their hive, and make provifion of it. Befides, they keep in the cells already made, the wax which is to be ufed to make others; or to ferve for any other purpofe.

2. The cells are the cradles for the young ones. But whence do thefe young ones come? It is one of the greateft difficulties relating to this fubject to make the difcovery.

The fabulous traveller who fpeaks of a nation, where there is no diftinction of fexes, and where he could not difcover how generation was per-formed, might have taken this thought from the bees; and *Virgil* was not in the wrong to praife their chaftity, or even to believe the fable of the bull, for want of a better. In a whole hive com-pofed of 8 or 10,000 bees, there is but one per-haps which breeds. It is longer, and of a more lively colour than the reft; it has a grave and a folemn deportment, and is what they commonly call *the king*. There are fometimes 2 of thefe, or at moft 3 in a hive, which has caufed a fufpicion,

that

that the priviledge of generation belongs only to one, for it appears from M. *Maraldi*'s obfervations, that it belongs only to this royal fpecies. The whole body of the people is condemned to barrennefs.

Moft commonly the king breeds his young in fuch parts of the hive where he cannot be obferved ; but when by good fortune he makes choice of others more expofed to view, it is ftill generally very difficult to fee him, becaufe the bees draw a curtain before him. This curtain is their ownfelves hung from top to bottom, and hooked to each other by little hooks that they have in their feet. By this means they can make fuch figures in the air as they pleafe. The king conceals himfelf thus ; either out of care for his young, or perhaps through modefty ; for there is nothing that we may not imagine of the bees. But however he has not always been able to efcape M. *Maraldi*'s eyes. He has perceived him to be followed by a court, moving with his folemn air, and depofiting in 8 or 10 cells, one after another, fo many little white worms, which are afterwards to be changed to bees. Whilft this is doing, it appears by fome particular motions of the bees which compofe his retinue, that they carefs, applaud, or encourage him. After this he retires into the inner part of the hive, from whence he feldom comes out.

By the 8 or 10 worms, which he depofits one after another, in the little time, and in the circumftances that have been obferved, we may judge of his fruitfulnefs during the reft of the time, when he is not perceived ; that is, during almoft the whole year. It muft needs be prodigious. When he is fingle in a hive, which is moft ufual, he is the only one that produces : there

comes

comes out of this hive in the compafs of a year, one fwarm at leaft, which may confift of 12 or 15,000 bees; fometimes there come out 2 or 3; and yet it is as full at the end of the fummer, as it was at the beginning of fpring. A new fwarm therefore, if it is the only one of the year, muft confift merely of the king's family, fuppofing there are none but young bees in it; and if there are any old ones in it, there remains in the hive an almoft equal number of the young ones, which were produced by the king, which comes to the fame thing. It is not probable that the king which comes out of the hive with the new fwarm, has produced any part of the bees which accompany him. But if there comes in a year more than one fwarm out of a hive, they muft ftill be new productions to be placed to the account of the old king, unlefs for fear of carrying his fruitfulnefs too far, we fhould fufpect that he has produced more than one king; that there went out but one with the firft fwarm, and that the other, or two others remained in the hive, and produced their young. If this be fo, a king may come forth with an entire new fwarm produced by himfelf, and fo be literally the father of his people; whereas the other kings will only be the brothers, becaufe they came from the fame bee. Which way foever we take it, they have this particular privilege, that their king is given them by nature herfelf.

It remains to inquire from whence his fruitfulnefs is derived, and whether it is from any copulation. There is hardly any hive wherein we do not find fome drones, and fometimes feveral hundreds of them. They are made like the bees, only they are about $\frac{1}{3}$ longer and thicker, and have no fting. They have none of the laborious temper of the bees, and remain abfolutely idle.

They

They go very little out of the hive, unlefs in very fine weather, and return again without bringing any thing with them. Not but that their veficle is filled with honey, but they are fufpected to have robbed the hive of it, becaufe they are not feen to fettle upon flowers. And even if they fhould go to gather any, it would be only for themfelves, and not for the publick good; for M. *Maraldi* has found, that upon fqueezing their veficle, there came no honey out of it, as there does out of the bees; and fo the drones are not capable of throwing it out again. We might imagine thefe animals to be the males of the great bee or king; and that they are fuffered in the hive only becaufe their idlenefs would be fufficiently recompenfed by this important function. And what would fupport this opinion, is, that at the end of the fummer the bees make war upon the drones without mercy, kill them, or drive them out of the hive without quarter, fo that we do not know what becomes of them afterwards. One would think that the caufe of their misfortune might be their being become abfolutely ufelefs, becaufe they do not perform the work of generation in winter. But what caufes a great deal of difficulty is, that M. *Maraldi* has feen fome combs without drones in fummer, and at a time when the cells were well fupplied with little worms.

The myftery of the generation of bees remains therefore pretty much concealed, but the care which they all take in common of their young ones which they have not brought forth, and which belong only to their king, is very vifible and remarkable. They feem to be regarded as children of the ftate. They give each little worm in its cell fome drops of liquor for its nourifhment, and then make a waxen cover for the cell.

I

Thefe

Thefe different operations have their regular times, and are without doubt for the ufes of the embyron. We leave the particulars of this to M. *Maraldi*, as alfo thofe of the fucceffive growths of the worm, which being at laft changed to a fly, pierces the lid of its cell, and after continuing fome time in a languid ftate, flies about with the reft. It is remarkable, that the bees carry their frugality to fuch a length, that they will not fuffer this broken lid to be loft. They take up the wax, and carry it into the common magazine, to be ufed anew; at the fame time they give the cell its regular figure, if it has been altered, and put it in a condition to ferve again for the fame purpofe, there have been worms fometimes running in the fame cell, in 3 months time.

The drones proceed from the king as well as the bees; there are in the combs cells larger than the reft, intended for the worms which are to change to drones, and confequently require more room; thefe worms are produced by the king with the fame ceremony, and afterwards treated by the public with the fame care, as thofe which are to be bees; all is equal 'till the end of fummer, but when the time is come for the bees to declare war againft the drones, their fury extends even to thofe which are yet but worms; they break the lids which they themfelves had put over them, and pull them out to kill them, and throw their little carcaffes out of the hive, a change very difficult to be comprehended in fo wife a nation.

We omit a great many particulars related by M. *Maraldi*, to mention all the wonders of this infect would carry us too far; and how many infects have their wonders yet unknown? and how many others have theirs, which will be unknown for ever?

*An*

*An explanation of the figures in plate* IV.

*Fig.* 1. The king, or rather queen of the bees in its natural bignefs.

2. The drone in its natural ftate.

3. The bee in its natural ftate.

4. The trunk of the bee extended in length, and larger than nature, with the 4 branches a little feparated from one another to fhew them the better.

5. The head of the bee to fhew its jaws.

6. The leg of the bee magnified, detached from the body of the bee, and loaded with wax.

7. The bafe of the cell in a horizontal fituation, to fhew more plainly the figure of the egg immediately after it is laid, and in what manner it is ufually placed upon its bafe.

8. The bafe of the cell in its vertical and natural fituation, with its egg changed into a worm or maggot, and furrounded with a fmall quantity of liquor 4 days after it is hatched.

9. The worm inlarged 8 days after it is hatched.

10. The fame worm 12 days after it is hatched, having changed its figure and fituation.

11. The fame worm changed into a *Chryfalis* larger than nature. It reprefents the bee while it is yet white and foft.

12. The figure of a cell detached from the reft and feen on the outfide.

13. A part of a comb which fhews in what manner the cells are ranged in the two oppofite faces of the comb.

14. A piece of comb which reprefents the cells feen on the infide, with the apertures ftrengthened with a rim.

15. Several cells from which the fides have been taken off only to fhew their bafes. This figure fhews how the bafes are ranged with regard to each other ; and in what manner the two orders of cells are formed in the 2 faces of the comb. For the angle A reprefents the concave folid angle at the bottom of the cell in one face of the comb ; the angle B, and the others of the fame order fhew the folid angle which is convex in the fame face of the comb, but concave in the oppofite face, and is found at the bottom of the cell oppofite to the firft.

16. Reprefents a canal, the origin of which is at A, where the 4 glandulous bodies are, and its extremity in B.

17. Reprefents a part of the fame canal much larger than nature, to fhew more diftinctly the two wings which are at A, the bag B, and the 2 ligaments CC.

18. The bag A larger than in the preceding figure, to diftinguifh thofe folds in which the feminal matter paffes.

19. Is the fame portion of the canal as in *fig.* 18. but feen on the other face, where are the 5 portions of black rings, and of the confiftence of horn, which embrace a part of the exterior circumference of the canal.

II. *On the progreffive motion of fome fhell-fifhes, or fea-animals.*

This is the continuation of a fubject begun in the hiftory of 1710*. Natural hiftory is immenfe, and that part of it which is expofed to our eyes,

* See vol. III. page 321 of this abridgment.

is nothing almoſt in compariſon of what is more concealed, and cannot be diſcovered without a great deal of time, leiſure, patience, ſkill, and a ſort of eyes which every body is not endowed with.

The razor-fiſh is an animal incloſed in a ſhell, almoſt after the ſame manner as a knife or razor is in a round ſheath. This ſhell is formed of 2 halves of a hollow cylinder, cut along its *axis*, or length, and theſe two pieces are joined on both ſides by a membrane which ſuffers them to recede a little, and approach again. The animal which inhabits this cylindrical ſhell, keeps itſelf always plunged perpendicularly in the ſand, but always with its head uppermoſt. Its head is diſcovered, not by its ſhape, but by two tubes, which receive and reject the water neceſſary for its reſpiration. The purr which was mentioned in the place above quoted, and ſeveral other ſhell-fiſhes or ſea-animals have ſuch tubes. The lower part of the razor-fiſh is that which ſerves for its progreſſive and perpendicular motion, for it only plunges itſelf into the ſand, or raiſes itſelf a little above it. For this purpoſe it has a ſort of leg which it thruſts out of its ſhell when it pleaſes, and it is cylindrical quite to its lower extremity ; or, when it is thruſt out, it becomes a ſort of ball, the diameter of which is greater than that of the cylinder. If the razor-fiſh has a mind to plunge, it thruſts out this whole leg, and conſequently engages in the ſand the great ball which terminates it, then it contracts the leg, the extremity of which being engaged in the ſand by a great ſurface, does not find it ſo eaſy to riſe again, as the ſhell does to go down. If the animal would raiſe itſelf, it only thruſts out at firſt the part which is

to

to become a ball; and then it endeavours to prolong the reft of the leg, or cylindrical part, and thruft it out, and this part refting upon the ball, cannot prolong it without making it go down, or pufhing the whole fhell upwards. But it is more eafy for the fhell to rife, than for the ball to go down; becaufe the ball refts upon the fand by too great a furface.

It is eafy to caufe thefe two motions in a razor-fifh. When the fea is retired, and has left the hole where it lodges uncovered, which is eafily found by its fhape, we need only throw a little falt upon it, and the razor-fifh will immediately come half out. It is then eafy to take it, but if we would fee it plunge again into its hole, we need but touch it, and it is the fame thing, if we mifs catching hold of it. After this, it is in vain to throw falt upon it, for it will not come out again. M. *de Reaumur*, the author of all thefe obfervations, thinks it is its averfion to the falt thrown upon it, and its endeavour to fhake it off, and get rid of it, that makes it come out of its hole. For he has found, that upon putting falt on thofe tubes or horns, with which it refpires the water, the little cylinders placed end to end, of which it is formed, feparate at their joints which have been touched by the falt, and fall upon the ground, or need only to be touched very lightly to make them fall, which deftroys an organ very neceffary for the animal. It is furprifing that falt fhould be fo injurious to an animal, which lives entirely in falt water.

The *dail* is another fort of fhell-fifh, which is always found plunged in the ooze, or in the *banche*, which is a foft ftone, but very hard in comparifon of the ooze; and M. *de Reaumur* proves it to

be

be nothing but ooze hardened, and petrified by the viscous part of the sea-water. The figure of the *dail*, and of its hole, is almost that of a truncated cone, the smallest base of which is always uppermost, and consequently the *dail* does not come out of this hole. It must have gone into it, or rather hollowed it when it was young, and afterwards have plunged it in farther, and augmented it in proportion as it grew. This is its whole progressive motion, which is only that of its growth; and therefore can be only of an extreme slowness. The instrument with which it bores is a pretty thick part, and made almost like a lozenge, and it thrusts it out at the lower end of its shell.

It must be imagined, that M. *de Reaumur* made use of some artifice to discover these sorts of operations, which are performed only in obscurity, and in secret. It was by holding a razor-fish in the air between his fingers, that he saw it thrust its leg out of the shell, and make the same efforts that it would have made to plunge itself into the sand; and it was by taking a *dail* out of its hole, and putting it into the ooze, that he saw the action of this part made like a lozenge.

All the young *dails* are in the ooze, and all the old ones in *banche*, which proves that the *banche* is ooze petrified. Very often the upper and greatest part of the hole is *banche*, and the rest is ooze. We may plainly see that the upper part, which receives the impression of the sea-waters more easily, must be the first that petrifies. It is highly probable that the *dails* live a long time, for the change of the ooze into *banche*, which is made during the life of a *dail*, must be made but slowly, and by insensible degrees.

The

The *dail* has alfo 2 tubes, with which it takes in the water, and throws it out again, and the length of thefe regulates the depth at which it keeps.

M. *de Reaumur* has obferved a fea-ftar with 5 rays, like that mentioned in the *Hiftory* of 1710*; but it has no legs to thefe rays, and differs alfo from the other in their being fhaped like lizards tails, which is its characteriftick. The 5 rays are themfelves the legs; the animal hooks two of them to the place to which it would advance, and trails itfelf upon thefe two, whilft the oppofite ray bending itfelf a contrary way, and refting upon the fand, pufhes the body of the ftar-fifh toward the fame place. There are two others which remain ufelefs, but they would not be fo if the animal had a mind to turn either to the right or left; and thus we fee how it might go on all fides with equal facility, only by ufing 3 of its legs or rays, and leaving the 2 others at reft. Perhaps alfo nature has given 5 legs to this animal, becaufe as M. *de Reaumur* has obferved, they are very brittle, and have occafion for a referve.

It is by a like mechanifm that the fea-urchin, or fea-hedge-hog walks with 2,100 fpines, with which its body is on all fides furrounded. It draws itfelf with thofe which are toward the fame place to which it would go, and pufhes itfelf towards the fame place with the oppofite ones, all the others remain without action during that motion. Which way foever it would place its body, it has legs to go in that pofition. It has the mouth however commonly downwards, that it may feed. Befides its 2,100 fpines or legs, there are 1,300 horns, which are of the fame ufe to it as the horns to a fnail, or a ftaff to a blind man, to feel the

* See Vol. III. page 323 of this abridgment.

ground

ground upon which it walks, and afterwards as anchors to a veffel to fix and hook it where it pleafes. It is plain, that as nature has fet its whole body round with fpines or prickles, the horns alfo ought to be prickly, fince the one cannot be in action without the others. M. *de Reaumur* has fubverted a fact related in the *Hiſtory* of 1709\*, on the credit of an able man, which however was falfe. This difficulty of obferving obfcure or complicated things fufficiently juftifies fome miftakes; but if miftakes were inexcufable, it would be ftill more inexcufable not to acknowledge them.

## *An Explanation of the figures.*

*Plate* IV. *fig.* 20. reprefents a heap of fand, GGGGKKK, which muft be imagined to be prolonged a good deal below KKK; in this heap are the figures 20, 21, 22.

*Fig.* 20. reprefents the aperture TTTT, *&c.* of the holes of the folen, called in *Englifh* the fheath, or razor-fifh.

*Fig.* 21. fhews a razor-fifh rifing above the fand, after the fifher has thrown fome falt into the aperture of one of the holes T, the flefhy part OO is then purfed up.

*Fig.* 22. fhews a razor-fifh rifing without compulfion above the furface of the fand, to refpire the water; the part which then comes out of the fhell, feems to be compofed of 2 tubes fet clofe together, AHC, aHc, the firft is bigger than the fecond; Aa are the apertures of thefe tubes, which in OO, *fig.* 21, feem almoft clofed, becaufe the animal would willingly hinder the entrance of the

\* Vol. III. Page 168 of this abridgment.

falt.

falt. CC, HH, ZZ, reprefent the places where the different portions of which the part Aa, CC is compofed, are united together.

*Fig.* 23. is the part Aa, CC, of *fig.* 22, which we muft fuppofed to have been detatched by the falt, applied at CC, Bb is the part which refted upon CC.

*Fig.* 24. is a razor-fifh laid upon the fand ; here we fee how it prepares itfelf to begin its progreffive motion. P is the extremity of the leg, which comes out of its fhell, and is then flatted. In the fame figure MMmm fhew the membrane, which on one fide is faftened to the edges of the 2 pieces of the fhell ; towards LN there is another membrane, which joins together the two other edges of the fame pieces, but this membrane could not be fhewn here, it appears in *fig.* 25.

*Fig.* 25. fhews alfo a razor-fifh laid upon the fand, but on another fide than that of *fig.* 24. at L is the fpring like that of the fhells of oyfters, and mufcles, which faftens the 2 pieces of the razor fifh together; from this fpring to the other extremity NN of the fhell, there is a membrane LNN made in form of an ifofceles triangle ; at R is the leg of the razor-fifh, already plunged in the fand, and hooked.

*Fig.* 26. reprefents a razor-fifh, which is ready to make one ftep to plunge itfelf into the fand, CCCC mark the fhell; from the lower aperture of this fhell proceeds the leg IRP, of which the part IR is cylindrical, at the end of this cylinder is the part P, which may be called the button of the leg, at OO are the fame 2 tubes, but more contracted, which appear at Aa, *Fig.* 22.

*Fig.* 27. is a razor-fifh of which the inner parts are fhewn, for it has been opened, after having cut the membrane MMmm of *fig.* 24. in two, this

very

very membrane is here wrinkled as may be feen in MmMm ; and its fpring alfo endeavours to fold it. L is the place where the leg LP is fufpended, which here has a very different form from that under which it appears in *fig.* 24, 25, & 26. It is compofed of circular and longitudinal fibres, which ferve to lengthen and contract it, to thicken and flatten it, according to the occafions of the fifh ; at EEII appears the membrane or membranes which form the tubes AHC, aHc of *Fig.* 22. here they are folded, which is probably their natural ftate.

*Fig.* 28. is that of one of the fhell fifhes called in *French Dails*, taken out of its hole. AGP is one of the two piees of it s fhell, at ED is the elaftic ligament, which faftens the two great pieces together ; DB is a third piece of the fhell much fmaller than the two others. At AICF is drawn a flefhy tubular part, which the animal prolongs or contracts in different circumftances ; tho' it feems to be but one fingle tube, it is in fome fort compofed of two different tubes ; a membrane, of which the extremity appears in C, like a fort of partition, divides the tube from one end to the other into 2 equal parts ; the bending AGP is called the bafe of the fhell, the animal however never refts upon this part of the fhell ; and it is called the bafe only becaufe it is oppofite to the elaftic ligament ED, and becaufe in another memoir, to exprefs ourfelves in a convenient manner, we have given the name of the bafe of the fhell, to the part oppofite to this ligament.

In *fig.* 29. is reprefented a bit of ftone or *banche*, inhabited by the *dails* ; this bit often confifts of ftone from QQQ to III, and the reft of ooze ; OO, *&c.* are the apertures by which the *dails* fend

out

out the flefhy tube, which they make ufe of to refpire the water, at TTK there are fome of thefe flefhy tubes out of their hole. The *dail*, of which we fee the end of the tube K, appears placed in its hole as it is there naturally, half the fides of this hole is carried away, the part AALLHHPP is the part oppofite to the part FEDB of *Fig.* 28; LLHH is a membrane, which joins together the two great pieces of the fhell; S is a flefhy part, with which it hollows the ooze, ZX is a hole of a younger *dail* than the *dail* KAAPP. V is the hole of another *dail*, of which we fee but a part.

*Fig.* 30. ABCDC is a bit of ooze placed horizontally as it was in the bottom of the fea. CDFE, FEGH, &c. are different horizontal *laminæ*, into which the bit of ooze divides in drying.

*Fig.* 31. reprefents the fame bit of ooze as *fig.* 3. but placed in fuch a manner, that the furfaces are vertical, which at the bottom of the fea were horizontal; the different *laminæ* alfo, into which it divides, are in a vertical plane.

*Fig.* 32. reprefents one of thofe fea-ftars, which we have called *ftars with rays like lizards tails,* feen on the upper part. PTTRR are the rays of this ftar. ABDCE is the mafs of its body, or if I may fo fpeak, of its back. The letters MM fhew fome of thofe extremely narrow membranes, which the ftar agitates in the water, and are hidden when it is out.

*Fig.* 33. is the fame ftar reverfed, its rays or legs, and its body are marked by the fame letters with the preceding figure, that it may be feen underneath; at S is its mouth or fucker.

*Fig.* 34. reprefents the skeleton of a fea-urchin or fea hedge-hog feen above; O is its fuperior aperture. The TTTTT mark the five great triangles, filled with the eminences, with which its

furface

furface is adorned, The t t t t t mark the five fmall triangles, and the 10 B's mark the perforated lifts, which feparate thefe triangles one from another. At mM one of thefe eminences, or *apophyfes* which are upon the skeleton is reprefented larger than nature.

*Fig.* 35. is the fame skeleton of the fea-urchin feen underneath; the aperture H is its mouth, at DD appear two forts of boney rings, out of each ring proceeds a tooth of the animal, and each of its teeth is alfo inclofed in a boney fheath.

In *Fig.* 36. is a piece of the upper furface of the fea-urchin, reprefented larger than nature, that the difpofition of the lifts of holes may appear better, and that the order of the holes, with which they are filled, may be perceived more diftinctly. BB are two of thefe lifts, which are at the fides of a fmall triangle t; each lift B is formed of two different rows of holes, fome RR are compofed of four holes, and the others S have but two. T is the great triangle, which follows the lift.

*Fig.* 37. is alfo a skeleton, but a part of it has been taken off, to fhew the infide; we may obferve in it the fame diftribution of the great and fmall triangles, and of the perforated lifts; we have alfo marked thefe parts with the fame letters as in the preceding figures; but there does not appear upon this inner furface any of the inequalities which are upon *fig.* 34, 35.

*Fig.* 38. reprefents a fea-urchin in motion, the letters EE,*&c.* mark the fpines with which it draws tfelf towards EE, *&c.* and KKK fome of the fpines with which it pufhes itfelf towards the fame fide, the letters eeee are much fmaller fpines; CCC,*&c.* are the horns with which it feels the bodies that prefent themfelves in its way. It may be obferved that the furface of this fea-urchin is in fome

meafure

meafure divided into different triangles, like its skeleton.

*Fig*. 39. fhews a fea-urchin at reft; it is reverfed; its mouth appears at BB, furnifhed with five teeth; at CCCCC there are feveral of its horns ftuck againft the ftone PP; I is a horn feparated.

## III. *On the declination of the needle, tranflated by Mr.* Chambers

M. *Delifle* has communicated feveral obfervations on the variation of the needle, which had been fent him by intelligent perfons, from different parts of the kingdom, the refult whereof is:

1ft. That the variation is continually greater to the eaftward of *Paris*, and fmaller to the weftward.

2dly. That from St. *Malo* to *Geneva*, which may be taken for the two extremities of *France*, in refpect of longitude the difference of variation is only $1° \frac{1}{2}$.

3dly. That the variation which is now north-weft and increafes every year, has increafed at *Geneva* much after the fame rate as at *Paris*. From the year 1703 to the year 1711, that is about 15' every year, and even that an irregularity obferved at *Paris* in 1710, when the variation only increafed 5', was alfo found at *Geneva*.

4thly. That from 1706 to 1711, the variation has increafed at feveral cities in *France*, much after the fame manner as at *Paris*.

To fee fo many appearances of regularity fo well maintained, 'tis hardly poffible to refift the expectation of a theory to come, tho' the example of feveral crude attempts, which have already been made, muft induce us to have patience 'till a fuf-

ficient

ficient number of obfervations have been made ; M. *Delifle* has given a little hiftory of what has paffed among the learned on this occafion; we fhall here only give a fhort abridgment thereof, and the rather as a great part of what can be faid on this head, is already found in the notes of Father *Gouye*, printed in the memoirs for the year 1692. It may fuffice for our prefent purpofe, to relate what additions M. *Delifle* has made thereto, whether they be hiftorical or philofophical.

The attractive faculty of the load-ftone was known by the ancients, but that other power whereby it points towards the pole was not known 'till many ages after, the earlieft author who fpeaks of it being a *French* poet of the thirteen century, and its variation came 300 years after ; the firft who publifh'd this was *Cabotto*, a *Venetian* navigator in 1549, tho' M. *Delifle* has a manufcript of a pilot at *Dieppe*, called *Crignon*, which he dedicated to the admiral *Chabot* in 1534, wherein mention is made of the variation of the needle ; this novelty turned all the philofophers againft it, as it broke in upon their fyftems, in effect they obftinately denied it ; but it growing at length inconteftable, they were forced to yield.

Hereupon it was obferved however, that under the meridians of the *Azores* there was no variation; this made them conclude, that they had found a natural principle, for fixing the firft meridian there, which hitherto had only been done arbitrarily, and confequently not to the liking of every body ; by the direction they found the load-ftone at poles, and by its variation that thefe were different from thofe of the earth, they therefore placed them where they pleafed, with intire liberty which was the fruit of a want of obfervations.

They

Pl. IV. *Vol. IV.* p.188.

Fig. 1. Fig. 2. Fig. 3. Fig. 5. Fig. 4. Fig. 6. Fig. 21. Fig. 23. Fig. 32. Fig. 33.

Fig. 7. Fig. 8. Fig. 9. Fig. 10. Fig. 11. Fig. 27. Fig. 22. Fig. 20.

Fig. 12. Fig. 13. Fig. 14. Fig. 15.

Fig. 16. Fig. 17. Fig. 19. Fig. 18. Fig. 26. Fig. 29. Fig. 39. Fig. 37.

Fig. 24. Fig. 28. Fig. 34. Fig. 38.

Fig. 25. Fig. 30. Fig. 31. Fig. 35. Fig. 36.

*J. Mynde Sc.*

Fig. 4.

Fig. 32.

p.188.

Fig. 15.

Fig. 37.

Fig. 38.

J. Mynde Sc.
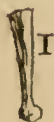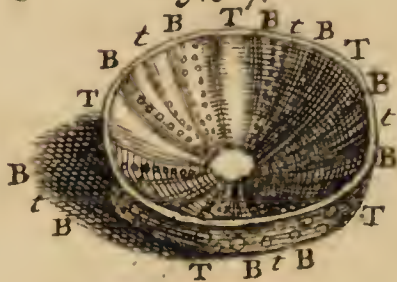
But they came at length to the knowledge of two new meridians, void of variation, one paffing through a cape fituate near the cape of *Good-Hope*, and for that reafon called the cape of *Needles* ; and the other thro' *Canton* in *China*; accordingly they determined the angles of interfection of thefe fuppofedly fixed meridians, the prefumption being ftill on the fide of immobility, and filled up their intervals with other meridians, under which there was a variation, and even ranging them proportionably on the fuppofition that every thing proceeded according to the moft natural order, but all this was direct precipitation.

Soon after it was difcovered, and principally by M. *Gaffendi*, that the variation of the needle had another variation ; that it was changing from time to time in the fame place, and this continually. A difcovery which overturned all they had built. By this inftance as well as by infinite others, it appears how flow our progrefs is, that there is always a confiderable diftance between one difcovery and another, and yet that thefe diftances grow continually fmaller and fmaller the nearer we defcend to the prefent time.

Of the ruins of fo many fyftems, nothing now remains but that propofed by Dr. *Halley*, who has drawn a line round the globe of the earth, which is exempt from variation ; this line is neither a meridian, nor a circle, but a very irregular curve. The variation of the declination in each place require this line to be movable ; and we already find by fufficient experience, that it is fo. 'Tis probable likewife that it changes its figure, by reafon the variations of the declination in one place are not always proportionable to thofe of another; this line, as drawn by Dr. *Halley*, paffes on the

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one fide thro' the north-fea, and the *Bermudas* iflands; and on the other, through *China* about 100 leagues to the eaft of *Canton*.

M. *D.lifle* from fome obfervations made by a *French* fhip which went through the *South-fea* to *China*, and was the firft fhip of that nation which fteered that courfe, has found another line of no variation which traverfes the *South-fea* from north to fouth, much like a meridian; this makes a confiderable addition to the fyftem, as well as the chart of Dr. *Halley*, wherein the *South-fea* was intirely wanting.

There is one remarkable difference between the 2 lines or portions of lines drawn by Dr. *Halley* and M. *Delifle*, when compared together, for to the eaftward of the line of no variation which paffes thro' the *Bermudas* iflands, the declination is north-weft, and weftward of the fame line northeaft, the contrary whereof obtains in the line which paffes thro' *China*; whereas in the other line, traverfing the *South-fea*, the declination is northeaft on both fides; this difference gives each of them its feveral character, which, if it be invariable, will always help to diftinguifh them whitherfoever they go.

M. *Delifle* has taken a deal of pains to difcover fome traces of the motion which thefe three lines muft have had to arrive at their prefent pofition; and is convinced that the line which now paffes thro' the *Bermudas* iflands is the fame with that which about the year 1600 paffed through the cape *des Aiguilles*, on which fuppofition it muft have moved from eaft to weft, though not parallelly to itfelf; for in the year 1600 it was almoft a meridian which paffed through the cape *des Aiguilles*, the *Morea*, and the *North-Cape*;

but

but from that time to this, it has moved 1400 leagues by its northern end, and only 500 by its fouthern one; fo that its prefent fituation is north-weft, and fouth-eaft; being much inclined to its antient pofition, its northern end paffed thro' *Vienna* in 1638, thro' *Paris* in 1666, and thro' *London* in 1667, which places were all according-ly free from variation in thofe years. M. *Delifle* is likewife of opinion, that the line which is now 100 leagues eaftward of *Canton*, is the fame which in 1600 paffed through that city; whence it fol-lows, that it has travelled from weft to eaft, contrary to the other, and at a much flower rate. If thefe 2 lines continue their courfe, they will at length meet one another, the confequence whereof it is not eafy to forefee.

As we have no antient obfervations of the *South-fea*, it would be prefumptuous to advance any thing as certain, relating to the line which paffes through it, only it might perhaps be fufpected to be the fame which formerly paffed through the *Azores*, and which has moved from eaft to weft.

The difference of the declination in different places M. *Delifle* obferves, is by no means proportionable to the diftances of thofe places from their line of no variation, that is, to a degree of dif-ference in the declination of the needle, corre-fpond very different diftances on the furface of the earth. In Dr. *Halley*'s chart, the greateft of thofe diftances is 130 leagues, and the fmalleft 15; but then he has only extended his chart to the 60th degree of northern latitude, and M. *Delifle* who has obfervations made 20°———more nor-therly, finding that there are fome degrees of difference in declination which only afford 8 leagues diftance; and hitherto it appears, that as

the climate is the more northern, the smaller diftance correfponds to a degree.

Nor does the declination vary equally in equal times at the fame place. At *Quebeck* M. *Caffini* found, that it had only vary'd ¼ a degree in 37 years, whereas by other obfervations in M. *Delifle's* hands it has varied a whole degree in 11 years.

But notwithftanding all this, we perceive a fort of progreffion, and fome appearance of regularity which is enough to prevent a philofopher from being difcouraged at the fight of fo many feeming anomolies.

## IV. *Of a cavern which is exceedingly cold in* summer.

It would be no great furprize to moft people, if you fhould tell them, that in a fubterraneous place, a cave for example, it is warm in winter, and cold in fummer, which they have met with a hundred times. It is a paradox however to philofophers, who know this experiment to be fallacious, and that in reality it is hotter in a cave in fummer, than in winter; but that the difference between the heat and cold is not near fo great as in the exterior air; and that this inequality of difference makes the cave appear warm in winter, when one goes into it from a colder air, or cold in fummer, when one goes into it from a warmer air. Therefore none but philofophers can be furprifed at a cavern of *Franche Comté*, where it is really very cold in fummer.

This cavern is 5 leagues to the eaft of *Befançon*, in that part of the province commonly called *Montagne*, and in a wood near the village of *Chaux*. It it at the foot of a rock 15 feet high;

it is 80 feet deep, 140 in length from the entrance to the oppofite fide, and 122 in breadth. It was in *Sept.* 1711, that M. *Billerez*, profeffor of anatomy and botany in the univerfity of *Befançon*, who fent this account to the academy, went down to examine it. He found, that the bottom of the den, which is flat, was ftill covered with ice, which was beginning to melt ; and he fays, three pyramids of ice 15 or 20 feet high, and 5 or 6 broad, which were now alfo much diminifhed. There began to come out of the top of the entrance a fog, which proceeds from it all the winter, and accompanies the thawing of this repofitory of ice. But the cold was ftill fo great, that unlefs they walked and agitated themfelves, there was no ftaying $\frac{1}{2}$ an hour without fhivering, and a thermometer, which out of the cavern was at 60 degrees, fell at 10; that is, to 10 degrees below a very great cold. The ice of this grotto is harder than that of the rivers, is mixed with fewer bubbles of air, and melts with more difficulty. It is the more in quantity, as the fummer is hotter.

M. *Billerez* has found the caufe of this *phænomenon*, by obferving that the lands in the neighbourhood, and efpecially thofe above the vault, are full of a nitrous falt, or natural fal armoniac. Thefe falts being put in motion by the heat of the fummer, mix more eafily with the waters, which flowing through the earth, and through the clefts of the rock, penetrate quite into the grotto. This mixture freezes them exactly in the fame manner as the artificial ice is made, and the cavern is in great, what the veffel in this operation is in little. Some coagulations, or ftony incruftations, which are found, efpecially oppofite to the opening expofed to the north, by which

more

more nitrous particles of air might enter, confirm this fyftem ftill more. It is faid, that there are rivers in *China* which freeze in fummer for the fame reafon.

## V. Of *a fpring which makes the teeth drop out.*

At *Senliffes*, a village near *Chevreufe*, fituated in a valley at the bottom of a hill, there is a publick fpring, which makes the teeth drop out, without any defluxion, pain, or bleeding. This effect cannot be afcribed to any thing elfe, for the air is very good and temperate, and the inhabitants are more robuft and healthy than elfewhere; only above half of them have loft their teeth. At firft they fhake in the mouth for feveral months like the clapper of a bell, and afterwards they fall out very naturally. The water accufed of this diforder is very brifk, it is very cold when drank at the well head, it is found to be hard when ufed in the kitchen, and it is faid to gripe thofe who are not ufed to it. M. *Aubry*, the curate of the place, who fent a barrel of this water to M. *Couplet*, with an ample account of all that relates to it, fays, he had been advifed not to make ufe of it till after boiling, which would caufe the ill quality of it to evaporate. He takes it to be a mineral, and conjectures, that it even contains fome mercury.

M. *Lemery* having examined it every way, and fubjected it to all the chymical trials, has not been able to difcover any thing particular in it. Only in evaporating a gallon of it over a gentle fire, there remained 12 grains of an acrid, fixed, alkaline falt, which feems to be very little for fuch a quantity of water. He found no indication of mercury. Befides, he has given water, in which

quick-

quickfilver had been infufed or boiled, to children for the worms ; but he never found that it loofened the the teeth. The ill quality therefore of the water of *Senliffes* is too fine and fubtile, to make a fenfible difcovery of itfelf to us.

It has been more eafy for M. *Lemery* to find a like example. He has recollected, that *Vitruvius* fpeaks of a fpring at *Sufa* in *Perfia*, the water of which makes the teeth of thofe who drink it drop out ; and it is remarkable, that he has feen at *Paris* a *Perfian* born in that very city of *Sufa*, who could take 7 or 8 teeth out of his mouth with his hand, and put them in again with the fame eafe. He had the fcurvy indeed ; and perhaps the fpring of *Senliffes* would give that difeafe, if the goodnefs of the air, and other favourable circumftances did not hinder.

### VI. *Of the rifing of the fea in the ftreights of* Dover, *when the tide ebbs.*

F. *Gouye* fays, that a failor had obferved with the plummet in the *Pas de Calais*, or ftreights of *Dover*, that the fea rofe at the time of the reflux. The reafon of a *phænomenon*, which appears fo odd, is that the waters, which retire from the coafts of *England*, meeting with thofe which retire at the fame time from the coafts of *France*, fwell againft each other, and raife the middle of the ftreight.

VII. *On the principal organ of vision, and on the structure of the optic nerve.*

The experiment of the cat immerged in water, had led M. *Mery* to a new explanation of the motions of the *iris* of the eye ; the fame experiment had taught him that the *retina* is as tranfparent as the humours of the eye themfelves ; and thence he had concluded, that it was not made to receive the picture of the objects, and that the *choroides* which is opake and placed behind it, is fitter for this office : But he had only hinted this thought, and did not pretend to go to the bottom of a queftion, which had before been handled with great fubtilty in 1668, between Meff. *Mariotte, Pecquet,* and *Perraut*. M. *Mariotte*, having made a curious difcovery of a place in the bottom of the eye, where vifion failed, maintained that the *choroides* is the principal organ of it. His obfervation is known to all, who have the leaft tincture of natural philofophy.

M. *de la Hire* having given another explanation of the experiment of the cat, than what M. *Mery* had given, occafionally took the part of the *retina* againft M. *Mery*, who fubftituted the *choroides* in its room.

M. *Mery* anfwered in the firft place to what related to the motions of the *iris*, the firft fubject of the whole difpute, and afterwards he came to the queftion of the *retina* and *choroides*.

Tho' M. *Mariotte*, who was the firft that maintained the rights of the *choroides*, has very well fupported them, and feems even to have remained in poffeffion of the field of battle, having anfwered every thing, and written laft, it muft be confeffed

that

that the *retina* has remained in poffeffion of being the principal organ of fight ; as this does not effect the general fyftem of vifion, perhaps thofe who doubt upon this queftion, or thofe who are per- fuaded in favour of the *choroides*, do not confider how difficult it is to change the old and common ftyle, by which the *retina* is eftablifhed. How- ever, as exactnefs cannot be abfolutely indifferent with regard to truth, we fhall here relate what is moft important and new in this difpute, for we fhall avoid repeating what was faid in M.*Mariotte's* time, of which the publick may eafily be informed.

The principal organ of fight is that whereon the image of the objects is painted, that is, which receives the *vertices* of different cones of rays, fent from different luminous or enlightened points, this organ muft alfo be fenfible.

The *retina* is a membrane formed by the expan- fion of the optic nerve, which is opened into fmall flender filaments, white like the nerve ; behind the *retina* is the *choroides*, which involves it, being another membrane, which is a continuation and extenfion of the *pia mater* ; it is black in men, birds, and fome other animals, but in feveral *fpe- cies* it has colours, and thofe very lively : it will not be amifs to obferve that the *retina* and the *cho- roides* are alfo involved by the opake *cornea*, which is a continuation of the *dura mater*.

It has appeared to the philofophers, that the *retina* had all the characters of the principal or- gan of fight. It is placed in the *focus* of the refrac- tions of the humours of the eye, and confequently receives the *vertices* of the cones of the rays ; it is .very fine, and confequently very fenfible, or rather fenfible to very fine impreffions, fuch as thofe of the rays; 'it draws its origin from a nerve, and is

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itfelf quite nervous, and they are perfuaded that the nerves are the vehicle of all the fenfations; laftly, it communicates with the fubftance of the brain, where all fenfations feem to terminate; as for the *choroides*, they have either been in no great doubt about its ufe, or thought that it ftopped the rays, which the great finenefs of the *retina* would fuffer to pafs, and that it did with regard to the *retina*, what tin does with regard to a looking-glafs, efpecially in thofe animals where it is black, becaufe black abforbs the light, and all that could pafs thro' the *retina* would only trouble the vifion, if it was not deadned.

The experiment of the cat immerged in water, has caufed M. *Mery* to have different notions: he faw that the *retina* abfolutely difappeared, as well as all the humours of the eye, but that the *choroides* appeared very diftinctly, and even with the lively colours, which it has in that animal; thence he judged the *retina* to be as tranfparent as the humours, and the *choroides* on the contrary to be o-pake, and confequently that the *retina* was not fit to terminate the cones of the rays, or to receive the images of the objects; that the light ought to go through it, and could not ftop but on the *choroides* which then became the principal organ of vifion. The black colour of the *choroides* in man is alfo very favourable to this opinion; it not only agrees with the principal organ of vifion, that the action of light fhould be terminated when it arrives there, and it is certain that this action is terminated in black, which abforbs the rays, and does not reflect them, but it alfo agrees with this organ, that the action of light fhould be ftronger there than any where elfe; and it is certain alfo, that as light is engaged in a black body, and does not come out of

it,

it, it caufes a greater vibration therein. Thence it comes that black bodies are more eafily fet on fire by the burning-glafs than white ones, every thing elfe being equal. The tranfparence of the *retina*, and the opacity of the *choroides* were not fo certainly known in the time of M. *Mariotte.*

He judged rightly, that the pofition of the *choroides* behind the *retina* made for his opinion, but he did not draw fo great an advantage from it as M. *Mery* did, who has obferved in the other fenfes the fame pofition of the principal organ behind a mean organ, which gives a happy and conclufive analogy. The cuticle, which is extended over the whole fkin is the mean organ of feeling, of which the fkin is the principal organ. It is the fame thing with regard to the fmell and tafte, which, with regard to the difpofition of the organs, are nothing but feeling. The *membrana tympani* is a membrane extended before the *tympanum* or drum of the ear, and clofes it ; and by its means the agitations of the air are tranfmitted to the *lamina fpiralis*, the principal organ of hearing, inclofed in the labyrinth, beyond the *tympanum*. It is true, the *membrana tympani* is not immediately applied to the *lamina fpiralis*, but it is placed before it, which is fufficient for the analogy. The *retina* therefore will be alfo no more than a mean organ, which will probably ferve to hinder too great an impreffion of the light on the *choroides*, or to preferve it, which is the fole ufe afcribed to the cuticle, with regard to the fkin.

But, according to M. *Mery*, there is more than all that. The *retina* is infenfible, becaufe it derives its origin from the medullary fubftance of the brain, which is infenfible alfo ; and the *choroides*,

*roides*, on the contrary, is very fenfible, becaufe it arifes from the *pia mater*, which is certainly very fenfible.

This proof, which fome queftioned, engaged M. *Mery* in a more particular examination of the ftructure of the organs of fight. He not only fhewed the academy, by diffection, that the *retina* and the *choroides* had the origins which he had pointed out, but added alfo an important difcovery concerning the optic nerve. This nerve is not compofed of filaments, as has been thought, and as the other nerves are; it is only a marrow inclofed in a canal, which may be eafily made to come out. So long as the nerve is within the fkull, the canal is formed only of the *pia mater*, and the marrow is contained in it in a lump : but the fame nerve, when it enters the orbit of the eye, takes a fecond covering, which comes to it from the *dura mater*, and is the *cornea* ; it is a new canal, which covers the firft externally, and there the marrow is inclofed in an infinite number of little cells, which communicate with each other. It is alfo with more difficulty fqueezed out of this fecond part of the optic nerve, than out of the firft. The cells of the marrow have alfo a relation to the cavernous bodies.

The ftructure of the optic nerve, unknown till now, does not permit the *retina* any longer to be a membrane; it is no more than a dilatation of the marrow, wrapped up with two membranes, and marrow is not a proper fubftance to be the feat of a fenfation. We hardly conceive that it can ferve for any other purpofe, than to filtrate or carry the neceffary fpirits ; but the vibration of the fenfe itfelf muft be made upon a part more fufceptible of a ftrong impreffion, more folid, and

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more firm ; and it has always been thought, that none but nervous filaments could receive this vibration, efpecially as they have always been allowed alfo to communicate it to the brain, the centre of the whole.

If the new ftructure of the optic nerve obliges us to remove to the *choroides* the office afcribed to the *retina*, it will greatly diforder all the common ideas, either vifion muft abfolutely terminate in the *choroides*, without going any farther; or if it does go farther, it fpreads itfelf through the whole *pia mater*. It muft be confeffed, that thefe confequences have their inconveniences; but it is true alfo, that whatfoever fyftem we take, if we purfue the fenfations to the end, and till they reach the foul, we lofe ourfelves, and fall into the immenfe *chaos*, which is between the body and the foul.

## VIII. *On the experiment of the eyes of the cat immerged in water.*

We faw in the preceding article, the principal part of the difpute between M. *de la Hire* and M. *Mery*, on the cat immerged in water. It has been another queftion between them, why the bottom of the cat's eyes appeared ftrongly illuminated in the water, and abfolutely difappeared in the air. M. *Mery* pretended, that when the animal was under the water, there entered more light into its eyes, becaufe its *cornea* was more flatted, and in fome meafure wrinkled by the moifture. But M. *de la Hire* has given a pretty natural reafon for it, taken from the principles of opticks.

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The *cornea* being in the air does the office of a mirrour, becaufe it is fmooth ; and of a convex mirrour, becaufe of its figure. It has its *focus* therefore beyond itfelf, which is alfo very vivid, and it reflects their own image to thofe who view it, which by its vivacity hinders them from feeing any other object beyond the *cornea*. But when the fame organ is in the water, the little difference, that there is between its denfity, and that of the water, makes them be phyfically homogeneous ; it is therefore no longer a convex mirrour, and it is only inftead of a plain furface of water, and we fee thro' it all that we fhould have feen thro' the water.

IX. *On the reproduction of fome parts of cray-*
*fifhes, lobfters, &c.*

Tho' the fyftem of the animal quite formed in the egg renders the generation of it intelligible, yet it does not ceafe to be very marvellous. But that in the room of an organical part which has been cut off, there fhould grow another quite like it is a fecond wonder, of a different order from the firft, and out of the fyftem of the eggs. And indeed the philofophers have refufed to believe fo furprifing a fact upon the credit of the vulgar, and they muft be allowed to be excufable, if the ftudy of nature ought to give them any confidence in their knowledge.

It is of the cray-fifhes, crabs, and lobfters, that the fifhermen have faid, that when they have loft any leg or claw, there came another in the room of it ; and M. *de Reaumur* has had the curiofity, the fkill, and the patience to verify it ; and this

is

is the refult of the obfervations which he has made chiefly on the cray-fifhes.

Thefe animals have 2 great legs before, terminated by 2 pinchers. Each of thefe legs has 5 articulations, or joints ; and I fuppofe with M. *de Reaumur*, that they are reckoned from the extremity of the leg, where the pinchers are. When the legs of the cray-fifhes are broken by any accident, in walking, which is pretty common, they are always broken at a future, which is near the 4th articulation , and in time there is renewed exactly what they have loft ; that is, a part of the leg which has 4 articulations ; the firft of which has the 2 pinchers, fo that the lofs is entirely repaired.

If the great leg of a lobfter is broken defignedly at the 4th or 5th articulation, the part taken off is always reftored, but not if the amputation was made at the 1ft, 2d, or 3d, articulation. The reproduction is then very rare, if things remained in this ftate ; but what is very furprifing, they do not remain fo. If at the end of fome days we examine the cray-fifhes, which have the legs broken at thefe unfortunate and barren articulations, we find they have them all broken at the 4th articulation ; and we may fufpect them to have performed this operation themfelves, being fure perhaps of having a new leg. The reproduction is beft performed at this 4th articulation.

The reproduced part is not only quite like, but equal at the end of a certain time to that which was amputated. Hence it comes, that we fee many cray-fifhes which have their 2 great legs unequal in various proportions. This fhews the age of the fmalleft.

If a reproduced part is amputated, there is a new reproduction formed.

The

The fummer, which is the only time of the year, when the cray-fifhes eat, is the moft favourable feafon for the reproduction. It is then performed in 4 or 5 weeks; whereas it would hardly be performed in 8 or 9 months in another feafon.

The fmall legs grow again like the great ones, but more rarely and more flowly.

The horns grow again alfo.

If we add to this all that has been faid of the cray-fifhes in 1709*, we fhall find that they furnifh feveral rare *phænomena*. It appears in general, that the moft wonderful of all animals, with regard to mechanifm, are thofe which refemble us the leaft.

*An explanation of the figures.*

Tho' the cray-fifh is an animal pretty well known, yet we have thought fit to engrave it, in order to fhew the pofition of its different parts.

*Plate* V. *Fig.* 1. reprefents a cray-fifh lying on its back, that the pofition of its legs may be feen.

P p 1, 2, 3, 4, 5, are its great claws, P p are the two pinchers: the little pincher p is articulated in p. At 1, is the firft articulation of the leg; at 2, the fecond; at 3, the third; at 4, the fourth; at 5, the fifth, or the place where it enters into the body of the animal.

*Fig.* 2. is a part of a cray-fifh reprefented larger than that of the preceding figure, for the better diftinguifhing of the articulations and futures of the legs. The fame letters of the preceding figure mark the fame parts. P p the two

* Vol. III. p. 244. of this abrigment.

pinchers 1, 2, 3, 4, 5, the five different articulations. S is the future where the leg of the crayfish naturally breaks, and where when broken, it is soonest reproduced. This future is more distinguished than the rest in the figure, because it was necessary to shew it ; but it is no more sensible in the cray-fish than in other futures ; b c d e is that part which we have called the great leg of the cray-fish : b c d e are the different joints of it.

*Fig.* 3, 4, 5, 6, 7, mark the different terms of growth of a leg broken at the future S of *fig.* 2. Each of these figures is what remained to the leg from the fifth joint. 5 marks in all these figures the place where this fifth joint was.

*Fig.* 3. A shews the end of the leg just as it appears immediately after being broken, or a day or two afterwards.

*Fig.* 4. B marks the end of the leg, when the membrane which covers it again, has taken a figure a little convex.

*Fig.* 5. C shews the little fleshy cones which come out of the end of this leg.

*Fig.* 2. D is the same cone considered some days later ; it is more long.

*Fig.* 6. E is the little fleshy cone, which begins to bend.

*Fig* 7. F G H is the same part of the leg ready to grow. It is then bent at G ; its position is like that of one of the legs of the cray-fish laid upon the back *fig.* 1. tho' the leg in this figure is still wrapped up in a membrane, yet the articulations are distinguished thro' it. They may be seen thro' the transparence of the membrane. If we look at the end H against a strong light, we shall perceive the separation of the 2 pinchers as it is represented in *fig.* 8.

*Fig.*

*Fig.* 10. reprefents a cray-fifh in the fituation in which it often is when it walks; TTTT is the great table of fhell which raifes itfelf firft, when the cray-fifh begins to mew; t t t, &c. are the little tables which hold together by different membranes; they are joined together in the fame manner when the craw-fifh has mewed.

*Fig.* 11. is the part of the ftomach of the cray-fifh, where the three teeth are, and the cartilages which fuftain them. The teeth of the middle B is of a different figure from that of the teeth D D.

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# ABRIDGMENT
### O F  T H E

PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1712.

I. *Observations on the rain, and upon the theemometer and barometer at the royal observatory during the year 1711, by M. de la Hire*[*].

THE quantity of rain-water, and melted snow, was in

| | Lin. | | | | Lin. | |
|---|---|---|---|---|---|---|
| Jan. | 8 | $\frac{1}{4}$ | July | 51 | $\frac{1}{8}$ | |
| Feb. | 51 | $\frac{2}{8}$ | Aug. | 20 | $\frac{1}{8}$ | |
| March | 18 | | Sept. | 24 | $\frac{1}{2}$ | |
| April | 20 | $\frac{2}{8}$ | Oct. | 31 | $\frac{1}{4}$ | |
| May | 32 | $\frac{1}{8}$ | Novem. | 21 | | |
| June | 8 | $\frac{3}{8}$ | Dec. | 15 | $\frac{3}{4}$ | |

The fum of the height of the water of the whole year was 302 lines, or 25 inches, 2 lines, which is much more than the mean years, which afford us only about 19 inches. It is pretty extraordinary, that this year fhould afford us fo much water, tho' it rained very little in *June* and *Auguft*, which with *July*, commonly furnifh as much as the 9 other months together; and the more as it did not rain at all from *Sept.* 3, to 19; and from *Sept.* 19, when it rained 11 lines to *Oct.* 19, it rained but 2 or 3 lines. But the great fnows of *Feb.* with the rains that followed them toward the

[*] Jan. 9. 1702.

end of the month, gave all at once a great quantity of water, which caufed a confiderable overflowing of the river ; but it was not fo great as that which happened in 1658 in *Feb.* the height of which is marked in the cloifter of the *celeftins* at *Paris.*

We cannot determine any thing certain on the height of the water to be furnifhed by a certain quantity of fnow, for fome is very rare, and fome very much condenfed.

*July* 28, 29, and 30, furnifhed about 31 lines of water ; and this was the greateft continual rain of the whole year. There was a little ftorm on the 28th in the evening.

My thermometer was at the higheft at 62 parts $\frac{1}{2}$, *June* 16, at fun-rifing ; and at two in the afternoon it was at 73 $\frac{1}{2}$ ; which does not mark a very great heat ; for I have feen it rife to 80. *July* 10, when the heats are ufually greateft, it was but in its mean ftate about fun-rifing, which is the time when I make all thefe obfervations. This very thermometer was at the loweft at 20 parts, *Feb.* 15 ; but two days afterwards it rofe again to 36. The mean ftate of the air marked by this thermometer, as it is at the bottom of the caves of the obfervatory, when it always continues at the fame height, is 48 of its parts or degrees. It begins only to freeze in the country, when this thermometer is at 32 ; fo that from the mean ftate to froft, it falls only 16 parts ; and confequently the degree of heat of the air, which fhall be as much above the mean ftate, as the degree of the beginning of the froft is below it, will be 64, as it was this year nearly about the morning of the day of the greateft heat ; and the greateft heat of this day at 2 in the afternoon, when the thermometer marked 73 $\frac{1}{2}$, was almoft as much

3                                          above

above the mean ftate, as the greateft cold of the year was below it, when the thermometer was at 20, at the fun-rifing, the coldeft time of the whole day.

The thermometer which I make ufe of every day for my obfervations, is a fimple barometer, which is placed at the top of the great hall of the obfervatory, wherein the quickfilver always keeps 3 lines lower than that which M. *Picard* made ufe of, and in which he perceived light upon agitating the quickfilver in the tube, which was a new *phœnomenon*. I cannot fufpect that there is any air in mine; for it gives light like the other, and I have filled it with a great deal of care. It muft be obferved, that in order to have the true height of the quickfilver in the tube, the frame of the barometer fhould be a little fhaken againft the wall where it is hung.

My barometer was at the higheft at 28 inches, 5 lines, *Jan.* 21; the fky was clear, with a moderate north wind near the ground; but the upper wind was eaft. And during this whole month, there was but little rain, and the barometer was always very high; for half this month it was above 28 inches. The fame barometer was at the loweft at 26 inches, 9 lines, $\frac{1}{3}$, *Dec.* 10; with a very violent hurricane, the wind being toward the fouth with but little rain. Thus the difference between the higheft and loweft ftate of the barometer was 1 inch, 7 lines, $\frac{2}{3}$, which is a little more than the common ftate; that is, 1 inch, 4 lines.

I obferve in general, that during this whole year, when the barometer was at 28 inches, or thereabout, which happened pretty often, it has rained little or nothing, which agrees with the general opinion, that the barometer predicts the

ferenity of the air ; and when it was at the loweft, there was always a good deal of rain and fnow, as it happened in *Feb.* This rule however is not fo certain, as to have no exceptions, for about the end of *July*, when it rained confiderably, as I have already obferved, the barometer was about 27 inches, 8 lines ; which may happen from particular caufes, which are not common, as a fudden ftorm, when we often fee 2 contrary winds, which having different directions either upwards or downwards, and lafting but a little while, make irregular impreffions on the quickfilver in the barometer.

The winds were this year as ufual in this country, very often toward the fouth-weft.

*Oct.* 6, at eight in the evening an earthquake was perceived only in my apartment at the obfervatory ; and one of the principal figns of it was, that the great rings of a copper ciftern ftruck againft the ciftern, made a good deal of noife, and continued a long time in motion, which was obferved by all who were in the place: but I did not mention it then, becaufe I fufpected that this ciftern, which had a good deal of water in it, might have flipped a little upon its frame, and that a fmall motion of the water might have given one to the ciftern, fufficient to make the rings ftrike againft the body. But fome days afterwards we received letters out of the country, 30 leagues from *Paris*, by which we have been informed of an earthquake which was felt there, and had terrified the people of the place; and it was on the fame day, and at the fame hour, that we perceived it at the obfervatory. We have had accounts of it alfo from other places, where it was very confiderable.

*Dec.* 30, 1711, I found the declination of the

needle

needle to be 10° 50' toward the weft as it was the preceding year; but it muft be obferved, that at the end of the year 1709, it was but 10° 15'; and confequently from 1709 to 1710, the variation has been 35 minutes, which was about double of what it was obferved to be for fome years; but 1711 has fet it right, for we have but 35 minutes difference for 2 years. We cannot however fufpect any error in thefe obfervations, for we always make them with very great care, ufing the fame needle, which is 8 inches long, and applying one fide of the fquare box, in which it is inclofed, againft one of the faces of a great ftone pillar at the bottom of the terrafs of the obfervatory. We are affured of the pofition of the face of this pillar, by feveral very exact obfervations, that it looks exactly to the weft.

II. *A comparifon of the obfervations made at* Zurick *on the rain, and on the barometer, with the foregoing, during the fame year*[\*].

M. *Scheuchzer* compares his obfervations of the quantity of water, both in rain and melted fnow, month by month, with what I have found at *Paris* at the obfervatory which I had fent him by order of the *Abbé Bignon*, whereby we find, that it rained more at *Zurick* than at *Paris* in every month, excepting only the month of *July*. For he found in *Jan.* 34 lines $\frac{1}{2}$; in *Feb.* 109 lines; in *March*, 44 lines $\frac{3}{4}$; in *April*, 26 lines $\frac{1}{2}$; in *May*, 39 lines $\frac{1}{4}$; in *June*, 15 lines; in *July*, 38 lines $\frac{3}{4}$; in *Aug.* 66 lines; in *Sept.* 35 lines $\frac{1}{2}$; in *Oct.* 62 lines $\frac{3}{4}$; in *Nov.* 43 lines $\frac{1}{4}$; and in *Dec.* 25 lines.

This gave him for the whole year, 45 inches, 1 line, *Paris* meafure; and he obferves, that it is

[\*] April 26, 1712.

one of the greateſt heights of water which has been hitherto obſerved : but at *Paris* I found only 25 inches 2 lines, which, however, is one of the greateſt that has been ſeen here. I have related in the obſervations of another year, ſome reaſons which may ſhew, that there muſt fall more water and ſnow in the mountainous countries, than in the plains which are diſtant from them.

He found the greateſt height of the barometer *Dec.* 22, at 27 inches; that day it was here at 28 inches, 3 lines, $\frac{1}{4}$, in the morning, with the common barometer ; but with another at 28 inches, 6 lines, $\frac{1}{2}$ ; the difference therefore was 18 lines, $\frac{1}{2}$. The leaſt height of his barometer, was *Feb.* 9. at 25 inches, 11 lines, $\frac{1}{2}$ ; and the ſame day here at 26 inches, 11 lines, $\frac{5}{6}$, with the common barometer, and with the other at 27 inches, 3 lines, and the difference with this laſt is but 15 lines, $\frac{1}{2}$ ; but thoſe days are not the ſame on which I here obſerved the greateſt and leaſt height of the barometer. Thus the difference between the greateſt and the leaſt height of the barometer, which I found at *Paris*, is 20 lines ; and he finds it but 12 lines $\frac{1}{2}$. This ſeems to me to ſhew, that the heights of the quickſilver in the barometers do not always come from the height of the whole atmoſphere, which cannot be very different in places upon the earth, which are not very far from each other ; and at the ſame time, but from ſome particular accident of the air. However, if we took a mean difference of the height of the barometer at *Zurick* and at *Paris* in the obſervations which I have juſt related, we ſhould have almoſt 17 lines; and if we ſuppoſe 11 toiſes of elevation for a line of alteration in the height of the quickſilver, it would follow, that

*Zurick*

*Zurick* would be 187 toifes higher than *Paris* above the fea.

He inlarges very much upon the meteors, and chiefly on the earthquake, which was felt at *Bafil*, of which M. *Bernoulli* has fent him a very exact relation ; and this is the refult of it.

There were two fhocks of this earthquake at but a little diftance from each other, *Feb.* 9, between four and five in the morning ; we felt one at *Paris*, *Oct.* 6, at eight in the evening, whence we fee, that the caufes of thefe earthquakes were very diftant. At *Bafil* the earthquake was preceded by a very violent fouth wind, like a tempeft, or a hurricane, which was accompanied by an extraordinary heat in this feafon of the year, tho' it was very cold before. At that time all the fnow, which was very deep upon the ground, was melted in lefs than two hours, and all the rivers, and the *Rhine* itfelf, increafed extraordinarily, which had not been feen till then ; but when this wind ceafed, the cold began again, and there fell a very great quantity of fnow, to the height of 2 cubits. Thefe are his own words. M. *Scheuchzer* obferves, that the fame accidents of wind, and heat, were alfo obferved at *Zurick*, but he does not fay, the earthquake was felt there, he only adds, that at the fame time he obferved the height of the barometer to be 25 inches, 11 lines.

In the laft place, he relates, that the fruits of the earth did not ripen well in his neighbourhood.

III.

III. *On the ebbing and flowing of the sea, by* M. Caffini, *junior\*; tranflated by Mr.* Chambers.

Men have long been enquiring the caufe of that continual motion of the fea, whereby its waters in the fpace of 24 hours rife, and advance, twice towards the fhore, and fall again ; or retire as often again therefrom.——The motion of the rifing water is called the flux, flowing, or flood, and its ftate when arrived at its utmoft height high water. On the contrary, the motion whereby it defcends, is called the ebb, or reflux, and when it ceafes to fall, low-water.

By this fucceffive change in the height of the fea-water when retired, frequently proves too low in moft parts of the ocean for veffels of a certain burthen to enter or come out ; feveral rocks, fand banks, *&c.* are alfo covered at certain times by the fea-water, which are left bare at others ; fo that it is of the utmoft importance for the fake of navigation, to be able to determine the periods of the flux and reflux, and to have an exact acquaintance with the feveral *phænomena* which obtain therein.

The memoir drawn up by the academy, and fent by the count *de Pontchartrane* to the king's profeffors of hydrography, in the ports of the ocean, has procured a great number of obfervations of the tide to be made with all the accuracy and fuccefs imaginable.————An account has already been given of thofe made at *Dunkirk* and *Havre*, to which we fubjoin feveral obfervations, and drew new rules from them. The bufinefs now is to learn whether the rules which agree to thofe two ports, be general and capable of being extended to the other ports.

\* April 6. 1712.                                    There

There is the more neceſſity for verifying theſe rules, as ſome of them go quite counter to what is commonly ſuppoſed, that the higheſt tides always happen about the days of the equinoxes. An opinion which ſeveral philoſophers have laid down as a certain rule, and ſtrove to give the phyſical reaſons thereof.

In order hereto, ſeveral more obſervations were required in other ports, like thoſe made at *Dunkirk* and *Havre*; wherein might likewiſe be expreſſed, the time of low-water, and the depth of the ſea in that ſtate; for as the times when the ſea is higheſt deſerve well to be known, as being the fitteſt for entring of ports; ſo thoſe when it is loweſt, are no leſs neceſſary to be known on account of their unfitneſſes for thoſe purpoſes. —— Something of this kind we have had occaſion to examine by a new journal of obſervations, both of high and low-water, made at *Breſt* by the ſieur *Montier de Longchamps*; it begins on the 10th of *June*, 1711, and ends on the laſt of *Jan.* 1712. The times of high-water for every day, both the morning and evening ones are here regularly noted with the height of water therein; and commonly alſo the time of the low-water, and the depth of water in that ſtate. To each of theſe obſervations care has been taken to affix the conſtitution of air, and the diſpoſition of the wind, which have a great ſhare in the accelerating, or retarding the tides, and the different heights obſerved therein.

By comparing the times of high-water in the new and full moons at *Breſt*, we find that the high-water obſerved to have come earlieſt was on the 26th of *September* 1711, at 3ʰ 13′ ½ in the morning; and that the lateſt was on the 25th of *December* at 4ʰ 30′ in the evening; ſo that there

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are inequalities in the time of the tides even in new and full moons, yet thefe inequalities may be in fome meafure reduced to a rule, by fuppofing the mean time of high-water at, *Breft* to be at $3^h$ $45'$, and making ufe of the rule prefcribed at *Dunkirk*, and *Havre*, which is to add $2'$ to the mean time of high water, for every hour that the time of new or full moon anticipates the mean time of high water, and to fubftract on the contrary 2 for each hour, that the times of new or full moon comes behind the mean time of high-water.

For an inftance, on the 28th of *Auguft* 1711, the day of full moon, it was found high-water at $4^h$ $6'$ in the evening, and the time of full moon, as noted that day in the ephemeris was at $5^h$ $8'$ in the morning, the difference between $5^h$ $8'$ in the morning, and $3^h$ $45'$ in the evening; the mean time of high-water at *Breft*, is $10^h$ $37'$; the double whereof 21 is the number of minutes to be added to $3^h$ $45'$, on account of the full moon's anticipating the mean time of high-water, the fum gives the true time of high-water on the 28th of *Auguft* 1711 at $4^h$ $6'$ in the evening, the fame as was actually obferved. So on the 9th of *December* 1711, the day of new moon, high-water was obferved at $3^h$ $29' \frac{1}{2}$ in the evening, in the *ephemeris* new moon is noted for that day at $11^h$ $15'$ in the evening, the difference between $3^h$ $45'$, and $11^h$ $15'$, is $7^h$ $30'$, the double whereof 15 is the number of minutes to be fubftracted from $3^h$ $45'$, on account of the times of full moons coming behind the mean time of high-water, the remainder gives the true time of high-water, on the 29th of *December* 1711 at $3^h$ $30'$ in the evening, within half a minute of what was actually obferved.

As to the tides in the quadratures, high-water is then found at *Breft*, nearly about the fame time,

tho'

tho' with inequalities fomewhat greater than thofe obferved in the new and full moons, which is a neceffary confequence, fuppofing the motion of the tides to depend on that of the moon, fince that planet is found by aftronomers fubject to greater inequalities in the quadratures than in conjunction or oppofition.

Taking a medium between thefe inequalities, we fhall find the mean time of high-water in the quadratures, at *Breft* at 8ʰ 57ʹ, and the true time of high-water will be determined at *Breft* for the days of quadrature, after the fame manner as was done for the days of new and full moon, with this only difference, that in lieu of two minutes, we muft add or fubftract 2½ from the mean time, for every hour the quadrature anticipates, or comes behind the mean time of high-water.

The mean time of high-water at *Breft*, in the new and full moons, being fixed at 3ʰ 45ʹ, and in the quadratures at 8ʰ 57ʹ, gives us the retardation of the tides from the new and full moons to the quadratures 5ʰ 12ʹ, precifely the fame as was obferved at *Dunkirk*, and within 2 minutes of what was determined at *Havre*; fo that we may take it for a general rule, that the interval between the times of the tide from new and full moon to the quadratures, is lefs than from the quadratures to the new and full moons ; and that the diurnal retardation of thefe tides goes in a regular kind of progreffion, tho' the terms of this progreffion do not fall upon the days of new and full moons, and quadratures, but one, two, or three days after ; it being obfervable that the effect of the feveral phafes of the moon upon the tides, is not communicated immediately ; and that the higheft tides ufually happen two days after new and full

moons, as the loweft ufually happen two days after the quadratures.

We have already obferved that the higheft tides do not always happen about the equinoxes, and all the loweft about the folaftices; but that the different heights of the tide, feem to have a nearer relation to the different diftances of the moon from the earth; this rule, which agreed with the obfervations made at *Dunkirk* and *Havre*, and which we venture to extend beyond thofe ports, is farther confirmed by the obfervations made at *Breft*; for 'tis obferved that in the high tides happening after new and full moon, the fea rifes much higher when the moon is near the earth, than when fhe is further diftant therefrom; and that in two fucceeding new moons, where the moon is at an equal diftance from the earth, the tide ufually rifes to the fame height.

For an inftance, on the 10th of *November* 1711, the day of new moon, the moon's diftance from the earth was 93600 of thofe parts, whereof the mean diftance is 100000, which is one of the fmaleft diftances that can happen; and accordingly the tide rofe that day 19 feet 3 inches. On the 25th of *November* following, the day of full moon, the moon's diftance from the earth was 106540 of the fame parts, which is one of the greateft diftances that can happen; and accordingly the height of the tide that day was only found 16 feet, 9 inches, which is lefs by 2 feet 4 inches, than when the moon was nearer the earth: and this pretty much in the fame proportion as the moon's diftance from the earth. On the contrary, the diftance of the moon from the earth being pretty equal on the full moon of the 29th of *July*, 1711, and the new moon enfuing on the 14th of *Auguft*, the higheft tides which followed on the

31 of *July*, and 16th of *August*, were found about the same pitch.

But the height of the tides, which in these observations seems to depend on the distance of the moon from the earth, does not suit so well with the rule of the equinoxes; for on the 26th of *September*, the day of full moon immediately following the autumnal equinox, high-water rose in the evening to 17 feet, 5 inches; and on the day following, which was that of the highest tide, it rose to 17 feet 6 inches; on the 12th of *October* next, the day of new moon, the height of the tide in the evening was found 19 feet, 5 inches; and the next day which was the time of highest tide, it rose in the evening to 19 feet 6 inches; which was higher by 2 feet than on the 27 of *September*; tho' according to the rule of the equinoxes this tide should have been lower than the former, which was nearer the autumnal equinox; but if regard be had to the moon's distance from the earth in these two observations, it will appear that the tide should have been lower on the 27th of *September*, than on the 13th of *October*, since the moon's distance from the earth in the full moon of *September* was 103970 greater by much, than in the new moon of *October*, when it was 94680.

This relation of the tides with the distance of the moon from the earth, not only holds in the high tides succeding new and full moons, but also in the lesser tides which follow the quadratures; for on the 6th of *August*, 1711, the day of the last quadrature of the moon, when her distance from the earth is 106300, which is one of the greatest that can happen, it was found on the 8th of *August* following the day of lowest tide, that the flood only rose 10 feet 10 inches 4 lines, which is one of the lowest that has been known.

On

On the 21ſt of *Auguſt* following, the day of the firſt quadrature, the moon's diſtance from the earth was 97700, which was much leſs than on the 8th of *Auguſt*, and accordingly on the 22d of *Auguſt*, the day of loweſt tide, it roſe to 12 foot 5 inches 6 lines, which was higher by 1 foot 7 inches than on the 8th of *Auguſt*. So on the 17th of *November* 1711, the day of the firſt quadrature, the moon's diſtance from the earth being nearly the ſame as on the 3d of *December*, in the ſame year the day of the laſt quadrature, the low tides immediately following thoſe phaſes were fonud about the ſame height.

It appears therefore beyond doubt, that the different diſtances of the moon from the earth is one of the chief cauſes of the different heights obſerved in the tides, which agrees likewiſe with Mr. *Childrey*'s obſervations, related in the *Philoſophical Tranſactions* for *October* 1670, where he gives an account of ſeveral high tides, which cauſed great inundations in *England*, and which all happened when the moon was *in perigeo*.

After laying down rules for determining the time of high-water, and the days of higheſt and loweſt tides, we proceed now to conſider what befalls in low-water, that is, after the ſea is withdrawn and ſunk to her loweſt ſtate, which is alſo obſerved twice a day.

It ſhould ſeem at firſt ſight, that the ebbing and flowing of the ſea being a ſucceſſive motion, the time of low-water ſhould be a medium between the times of high-water immediately preceding and following it; but by all obſervation the ſea employs more time in falling than in riſing; the reaſon may be, that the force which obliges the ſea to riſe ſtill ſubſiſts for ſome time after it has arrived at its utmoſt height, and thus

keeping

keeping the water in fome meafure fufpended, prevents their falling with fo much velocity, as they would do if nothing hindered their defcent.

It follows hence, that to enter a port, 'tis by no means fafe to come before the tide, fince at equal diftances of time the fea is lower before, than after high-water.

But what is more remarkable in low-water is, that the higher the fea had rofe, the lower it finks again; and the lefs high the tide had been, the lefs low will it be after the ebb.——For an inftance, on the 13th of *Oct.* 1711, the day of the higheft tide at *Breft*, the water rofe 19 feet, 6 inches, above the fixed point; but upon the fea's withdrawing, it was found 1 foot, 10 inches, below the fome point; fo that the fea had fallen that day 21 feet, 4 inches.————On the contrary, on the 6th of *Sept.* the day of the loweft tide at *Breft*, high-water was found at 10 feet, 3 inches; and low-water, on the day preceding, was 5 feet, 11 inches; fo that the fea had only fallen that day 4 feet, 4 inches.

Hence it appears more eafy to determine the time of high-water about new and full moons, than about the quadratures; fince, in a fpace of time almoft equal, the fea fometimes rifes 5 times more in the new and full moons, than the quadratures.

From thefe obfervations it likewife appears, of how much importance it is to know the time of high-water in new and full moons, fince on thefe occafions the fea-rifes or falls with more rapidity than about the quadratures; fo that to bring a veffel into a port where there is only depth of water fufficient at the time of new and full moon, might be dangerous, if the pilot were

not

not well informed of the precife time of high-water.

From thefe obfervations new rules might be laid down for determining the time of high and low-water, and the height thereof at *Breft*, for all times of the year; and thefe rules being added to thofe already prefcribed for finding the time and height of high-water at *Dunkirk*, and *Havre*, will include almoft every thing to be defired on this head, for the advantage of navigation.

Tho' our intention be not here to give a general theory of the ebbing and flowing of the fea, yet we fhall here fubjoin our fentiment as to the caufe of the principal phœnomena obferved therein.

In order to this, we fuppofe, that the ebbing and flowing of the fea, may arife from the preffion of the fun and moon, upon the cœleftial matter which furrounds the earth; but more from the preffion of the moon which is nearer us, than that of the fun, which is farther off.

Now in the new and full moons, when the fun and moon are nearly in the fame direction with regard to the earth, thofe two luminaries poffeffing a place in the matter which furrounds the earth, neceffarily comprefs that matter, and hereby prefs upon the water in the fea, which being obliged to give way, and flow on either fide from the place of preffure to the diftance of 90°, where the fea muft be at its greateft height; in other fituations of the moon from the fun, the moon's action being directed differently from that of the fun, the preffure upon the earth muft be lefs, and confequently the fea lefs high in its flux, as well as lefs low in its reflux; laftly, the effect of the fun being oppofite to that of the moon in the quadratures, the preffure occafioned by the moon

muft

muft be partly deftroyed ; and, confequently, the fea be higher in the time of low-water ; but lower in the time of high-water, as is actually obferved.

As to the different diftances of the moon from the earth, they muft have a fenfible effect on the heights of the tides ; for the preffion of the moon upon the earth, muft needs be greater when near the earth, than when further off. Since the motion communicated in a fluid near at hand, acts more forcibly than that communicated at a greater diftance, that it may be obferved in a fluid put into motion, whereof thofe parts neareft the place where the motion commences are more agitated than thofe further off.

IV. *Reflections upon obfervations of the barometer made by M.* Vallerius *director of feveral copper mines in* Sweden, *by M. de la Hire, jun. tranflated by Mr.* Chambers*.

Shewing M. *Vallerius*, who is a good mathematician, when he was fome years ago at *Paris*, the changes which the mercury in the barometer undergoes between the top and bottom of the obfervatory, I defired him to make the fame experiments in the mines under his care, which he did accordingly laft fummer in the wells of *Flemengienus* and *Flemingffchatet*, and the mines called *Falhunenfes* in the great copper mountain, and upon the mountain *Grufriis-barget* adjoining to thofe mines the heavens being very cloudy, and the wind pretty ftrong, fo as to abate the heat.

He began his experiments with an obfervation of the barometer at the entrance of the mine, where

* March 19 1712.

where he found the mercury at the 24th, 10th, and the fourth 100th of the *Swedish* foot, which, in *Paris* meafure, are equivalent to 26 inches, 9 lines, $\frac{52}{100}$ of a line——Here it may be proper to obferve, that the *Swedes* divide their feet into 12 parts, each 10th into 10 more, which they call lines, and each line into 10 parts.

Then defcending with the barometer into one of thefe mines 45 *Swedish* fathom deep equivalent to 41 fathoms, 1 foot, 2 inches, 1 line, $\frac{1}{2}$, *Paris* meafure, he found the mercury ftanding at the 24th tenth, 7 lines, anfwering to 27 *Paris* inches, 1 line, and $\frac{26}{100}$ of a line; confequently the mercury had arofe 3 *Swedish* lines for 45 of their fathoms deep, equal in to 3 lines $\frac{424}{300}$ of a line for 41 fathoms, 1 foot 2 inches, 1 line $\frac{1}{2}$ *Paris* meafure.

He continued defcending 45 *Swedish* fathoms more, which is the loweft he could go, and obferving the barometer here, found the mercury at the 25ht tenth; fo that it had arofe 3 *Swedish* lines, as in the former 45 fathoms, *viz.* to 27 inches, 5 lines *Paris* meafure. Hence for 90 *Swedish* fathoms we find 6 lines difference in the height of the mercury, which amount to 7 lines, $\frac{448}{500}$ for 82 fathoms, 2 feet, 4 inches, 3 lines, *Paris* meafure.

But to be further affured of the accuracy of his obfervations, he made 2 in his return up again, whereby the whole depth was divided into 3 equal parts; whereas in going down, he had only made one in the middle; after afcending 30 *Swedish* fathoms, he found the mercury funk 2 *Swedish* lines, which anfwers to 2 lines $\frac{316}{500}$, for 27 fathoms, 2 feet, 6 inches, 9 lines, *Paris* meafure.——Afcending 30 *Swedish* fathoms further, he found the mercury funk 2 *Swedish* lines. ——Afcending 30 *Swedish* fathoms further,

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he found the mercury funk 2 *Swedish* lines.———
And laftly, arriving at the entrance of the mines,
which is 30 fathom further, he found the mercu-
ry funk 2 *Swedish* lines lower, fo that it now ftood
at the 24th tenth, and 4 lines, as at firft.

Not contented with thefe obfervations, M. *Val-*
*lerius* made others on the mountain *Grufriis-ber-*
*get*, adjoyning to the mine juft mentioned, and
afcending the mountain 15 *Swedish* fathoms per-
pendicularly high, he found the mercury 1 *Swe-*
*dish* line lower than at the foot of the mountain,
or the entrance of the mine, which amounts to 1
line, $\frac{1}{5}\frac{6}{0}\frac{8}{0}$, for 13 fathoms, 4 feet, 3 inches, 4
lines $\frac{1}{2}$ *Paris* meafure————Continuing to af-
cend 15 *Swedish* fathoms higher, he found the
mercury 1 *Swedish* line lower than in the former
obfervation———and arriving at length at the top
of the mountain, which was 22 *Swedish* fathoms
higher than in the former obfervation, and con-
fequently 52 *Swedish* fathoms above the entrance
of the mine, he found the mercury funk 1 *Swedish*
line $\frac{2}{10}$, fo that it now ftood at the 24th decad,
and $\frac{8}{10}$ of a line, and thus for 52 *Swedish* fathoms
had funk 3 lines $\frac{2}{10}$, which in *Paris* meafure
makes 4 lines $\frac{1}{5}\frac{0}{0}\frac{6}{0}$, for 47 fathom 3 feet, 2 inches,
10 lines $\frac{8}{10}$.

Defcending the mountain again, he obferved
the heights of the mercury in the fame places as
before, and found the fame differences, whence he
infers, that 9 lines, and $\frac{2}{10}$ of mercury, correfpond
to 142 fathoms of air, *Swedish* meafure, which
amounts to 12 lines $\frac{5}{5}\frac{4}{0}\frac{4}{0}$, for 129 fathoms, 4 feet,
11 inches, 1 line, and $\frac{1}{5}\frac{5}{0}\frac{0}{0}$.

For the more exactnefs of his obfervations, M.
*Vallerius* informs me that he made them with two
barometers which perfectly agreed from beginning
to ending.

By

By examining the obfervations above related, that from the bottom of the mine, to 27 fathoms, 2 feet, 6 inches 9 lines perpendicular height, above the top of it, there are 109 fathoms, 4 feet, 3 inches, for which the mercury funk 10 lines, and $\frac{564}{500}$, fo that there was always a line difference in the height of the mercury, to 10 fathoms, 1 foot, 6 inches, 4 lines, the mercury at the bottom of the mine ftanding at 27 inches, 5 lines, and at the top of the mountain 109 fathoms, at 26 inches, 6 lines, and $\frac{216}{500}$.

Thefe I take to be the firft experiments of the barometer, in places of this depth, which makes them of the more confequence, as they fhew that the fame differences in the height of the mercury, correfpond to the fame height of air, whether it be upon a mountain, or within the ground, even in a deep mine, where one might imagine that the great plenty of vapours, muft have made the air confiderably heavier than elfewhere.

Now upon comparing the obfervations of M. *Vallerius*, with thofe made in *France*, it appears that a line difference in the height of mercury in *Sweden*, correfponds to a lefs height of air, than the fame is found to do by Meff. *Caffini*, *Picard*, and *de la Hire* in this country.

For M. *Caffini* at the foot of the mountain *Notre-Dame de la Garde*, near *Toulon*, found his barometer at 28 inches, which after afcending to the top, 178 fathoms 2 feet, was funk to 26 inches 8 lines, fo that 178 fathom 2 feet, gave a diminution of 16 lines, which is 10 fathoms 5 feet per line, fuppofing the air equally denfe in this whole height———M. *Picard* again on *Mount St. Michael* found his barometer fink from the *Greve*, to the dial on the middle of the church, 4 lines $\frac{1}{2}$, the depth between which places is 64 fathoms, whence

whence he infers, that a line difference in the height of mercury anfwers to 14 fathoms, 1 foot, 4 inches.———Laftly, my father on the mount *Clairet*, by *Toulon*, found his barometer at 26 inches, 4 lines, $\frac{1}{2}$ ; but defcending to the banks of the fea he found it at 28 inches, 2 lines, which is 21 lines $\frac{1}{2}$ for 257 fathoms depth, or 12 fathoms per line ; and from the like experiments at *Meudon*, he found 6 lines, $\frac{3}{4}$, finking of mercury, for 85 fathoms, 2 feet, height of air, which gives 12 fathoms, and 4 feet, nearly for a line ; by other experiments made at the obfervatory, he found a line of mercury anfwer to 12 fathoms, 2 feet and $\frac{2}{3}$.

M. *Caffini*, jun. in the memoirs for the year 1705[*], gives a table of the heights of air correfponding to the heights of mercury in the barometer, founded on a rule eftablifhed by M. *Maraldi*, wherein it appears, that at 27 inches, 5 lines, the height of mercury found by M. *Vallerius* at the bottom of his mine, 11 fathoms, 1 foot, correfpond to a line difference of height of mercury ; and that at 26 inches, 6 lines, which is nearly the height M. *Vallerius* was at on the mountain *Grufriisberget*, 13 fathoms of air anfwer to a line of mercury.

The above-mentioned heights correfponding to a line of mercury are all greater than that found by M. *Vallerius*, tho' they fhould be fmaller, fuppofing as we may probably do, that the air is lefs and lefs denfe the further it is off the earth, fince they are all founded on experiments which began at a point where the barometer ftood at 28 inches; whereas that of M. *Vallerius*, when loweft, was only at 27 inches, 5 lines.———But as this proof

[*] Page

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may not feem convincing, I fhall fubjoin another founded on the fame principle, *viz.* that the air is an heavy elaftic body.

If the atmofphere * C X, which I fuppofe of a certain extent from the centre of the earth C, be divided from the top fo as to give all the points X, U, T, S, &c. where the mercury changes a line in its height, thefe points will mark fpaces which go continually diminifhing in a certain proportion.

If now a barometer be placed in any part of the atmofphere, thus divided as in R, and the mercury be found for inftance at 26 inches; if we would have it to fink a line, we muft raife it to S; but if while the barometer ftands in R, the atmofphere growing higher, makes the mercury rife 27 inches. To make the mercury now fall a line, we muft not raife it the height RS, but the height EF; for that the different changes of the atmofphere, produce the fame effects on a barometer which keeps it place, as would be produced by removing it into a higher, or lower place, while the air undergoes no alteration.

But by the experiments of M. *Vallerius* we find, that at a certain height of the barometer, a fmaller height of air anfwers to a line of mercury in *Sweeden* than in *France*, than is found at a greater height of the barometer in *France*, which fhews, that the atmofphere is much higher in the northern countries than 'tis here; and hence it feems to follow, that the perpendicular height correfponding to a line of mercury at a greater height of the barometer in *France*.

It had been conjectured from the obfervation of M. *Richer*, at *Cayenne*, that the atmofphere

* Plate V. Fig. 12.

be-

becomes higher as it is further from the line, the greateſt height of mercury which he found there during a whole year, being only 27 inches, 1 line ; whereas, at the obſervatory, no year paſſes wherein it does not riſe above 28 inches.

But the atmoſphere being higher in the northern than the ſouthern countries, the refraction muſt be much greater, which we find accordingly from the obſervations of ſome aſtronomers who attended king *Charles* II. of *Sweden*, in an expedition towards the north pole.

It remains to account for the differences between the obſervations made in theſe countries, for determining the perpendicular height of air correſponding to a line of mercury.————After ſifting the ſeveral changes which may befall the air, I apprehend this difference owing to two principal cauſes : the firſt, to a greater or leſs height of the atmoſphere ; and the ſecond, to trains of vapours diffuſed thro' the air near the earth, which on ſome occaſions may make the ſpaces of the air : they are in heavier without any conſiderable alteration in the height of the whole atmoſphere.

V. *A continuation of the obſervations on the bezoars, by M. Geoffroy, junior**.*

In my firſt obſervations I have remarked, that there is generally in the centre of each bezoar ſome foreign body, about which the bezoardic *ſtrata* are formed and diſpoſed. It has alſo appeared to me, that it might be a ſign, that the ſtones are not counterfeited, as thoſe who ſhould endeavour to counterfeit them, would not think of uſing a

* July 6, 1712.

pre-

precaution, which would be of no fervice to them ; and befides, they would never take the pains to feek for fo great a variety of fubftances, as thofe which ferve for a bafe to the different bezoar ftones.

The very foffil bezoars are formed after the fame manner. *Boccone* has obferved kernels in them of different forts, pebbles, gravel, wood, metal, coals, &c. I have examined that fort, called *Priapolites*, which grows in *Languedoc* ; and one of them was given me by M. *Bon* ; the centre of which is occupied by a piece of rock cryftal.

Among the different kernels found in the animal bezoar ftones, I have obferved one which appeared to me to refemble the ftones of *Caffia* or *Tamarind*, but fmaller. I have, however, found fince, that it might be the fruit of a pod, which I had not feen before, refembling that of the pod of the tree, called, *Acacia vera Ægyptiana*. This tree grows in *Egypt, Arabia*, and other places. The pod, which is brought to us from *Senegal*, is 3, or 3 ½ inches long, and 9 or 10 lines broad ; it is compofed of an outer ,and an inner membrane. The outer membrane is very tender, of a brown colour, and faftened to the inner one, which is cartilaginous, and very thin. The matter, which unites them, is gummy, of a tranfparent yellowifh colour ; it melts in the mouth, and has a very rough tafte. In the longeft pods I have found 8 grains feparated from one another by a fort of contraction, which reunites the fides of the membrance. Each cavity of thefe pods contains a flat grain, refembling a lupine, fometimes exactly circular, and fometimes a little compreffed by the contraction of the pod, which

is

is clofer in the middle than at the two ends ; fo, that the fruits of the middle of the pod are a little comprefled, and thofe of the end are exactly round.

What made me judge that thefe fruits were thofe which I had obferved in the bezoar, which is round, and a little flatted, is that I have found them to have the fame marks, and among many others, a circular whitifh line, drawn upon each face of the fruit, fuch as appears upon that which is found inclofed in the bezoar. I put fome of thefe fruits in water, they fwelled almoft in the fame manner as they would have done when found in the ftomach of the animal, where they began to be covered with the bezoartic matter. The tincture which I drew from thefe fruits was red and very four. I threw a little vitriol into it, and it grew black : thefe feeds, and their pods are ufed in the country where they grow, to tan leather. From their decoction in water is drawn a juice, which is thickened, and brought to us under the name of *Succus Acaciæ.* It is alfo pretended, that from this *Acacia* tree, that the gum is obtained, which we call gum arabic, and gum fenegal. Is there any probability that the makers of the bezoar fhould go to look among other things for the fruit of the *Acacia*, to make one of the bafes of their compofition ? And is it not more likely that thefe fruits and fome others, which ferve for nourifhment to the animals, caufe by their aftriction a thickening of liquors in the ftomachs of thofe animals, which eat the moft of it ; this thickening of liquor may caufe the formation of the bezoar ftones.

This is the manner in which thofe ftones grow in the ftomach of the animal that bears them, and

grow

grow to the ftate in which we find them. There may be feveral found in the ventricle of a fingle animal. *Tavernier* fays exprefly, that fix of thefe goats which he had for a prefent, had in all 17 bezoars, that they might be felt on the outfide and counted, which increafed the price of thefe animals, in proportion to the number of bezoars that were felt in them. This agrees perfectly with what *Clufius* relates of the animals which afford the occidental bezoar. He fays, that a friend of his in *Peru*, who had firft made the difcovery of the occidental bezoar, being defirous to know how thefe ftones were formed in the body of thefe animals, diffected one of them, and found in the ftomach a fort of pouch, where thefe ftones were ranged in order, as the buttons upon a coat.

These two paffages are quite oppofite to what *Pomet* tells us, that only one bezoar is found in the belly of each animal. He alfo affures us, that he would not have ventured to contradict the authors, who have treated of it; had he not had a piece in his hands to juftify his opinion.

It will be proper to examine it here, as nobody, that I know of, has publickly expofed *Pomet*'s error concerning the pretended coat of the animal *bezoar*, which he faid was one of the greateft curiofities that had been feen for a long time in *France* in the opinion of all judicious perfons.

*This coat is,* fays he\*, *of the bignefs of a goofe egg, having on the outfide a fhort rough hair, of a tawny colour, which being cut in two, difcovers a thin brown fhell, which covers another white fhell, as hard as a bone, in which the ftone is contained, that is called the* bezoar.

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\* *Pomet in his traité des drogues, livre des anim.* pag. 10.

Now

Now this fo fingular a covering of the *bezoar*, of which he pretended to have made the difcovery, is not any part of the animal which bears the *bezoar*, but an exotic fruit, in which either *Pomet*, or fome quack, whom he had fuffered to impofe upon him, had very artfully inclofed a *bezoar* ftone. This fraud was not difcovered 'till about a year ago. As I was to examine this fingular drug of M. *Pomet's* in company with M. *Vaillant* and M. *de Juffieu*, demonftrators of the plants in the royal garden, we perceived that this pretended covering could not be a part of any animal, and that it muft be fome fruit but little known : This was afterwards verified by M. *Vaillant*, who found he had fome of thefe fruits, and could make *bezoars* of them, with their coverings, juft like the *bezoars* fo much efteemed by *Pomet* ; and I have made fome of them myfelf. This fruit grows upon a fort of palm-tree defcribed by *John Bauhinus*, and called by him *Palma cuciofera* : this fruit is alfo defcribed by *Theophraftus*. The tree grows in *Egypt*, *Nubia* and *Ethiopia*. *Cordus* calls it *Nux Indica minor*, and has given a defcription of the fruit, fuch as I have juft now related from *Pomet*, in fpeaking of the coat of the *bezoar*. This defcription wants only one particularity omitted by *Pomet*, which is the fkin that again covers the whole fruit, and is of a tawny yellow colour; the fruit has a footftalk divided into 6 parts, 3 great ones, and 3 fmall ones. This would have been fufficient to have undeceived him, or thofe who have been deceived after him, and it is of fervice toward the perfection of natural hiftory, that frauds of this kind fhould be carefully revealed.

It is not without reafon that I put in my laft memoir in the rank of *bezoars* all the fubftances

that are formed in *strata* in the bodies of animals. The pearls which I reckoned in this number, deserve it so much the better, as I have found some in certain shells, so like the common *bezoar*, that they are hard to be distinguished at first sight: These pearls are engendered in a sort of shell-fish, called *Pinna Marina, Pinna sive Astura Mathioli;* we see a great quantity of them on the coasts of *Provence*, where they fish in *April* and *May*; this sort of fish is called *nacre* in that country.

The pearls which are found in these shells, are all of the same water; some are, as I have said, perfectly like *bezoar* stones; others of a coral, and amber colour, and others of a pearl colour, but more leaden; the most usual shape of them is that of a pear. All these varieties of shape and colour do not hinder them from being of the same nature, since they grow in the body of the same fish; I have four of a different water and shape, which were taken out of the same shell. That these, and all other pearls are formed in the body of shell-fishes, as the common *bezoar* is in the goats that furnish it, is not difficult to prove, since upon breaking them they are found to have stripes like certain *bezoars* already mentioned, and formed about a *nucleus* or kernel, which seems itself to be a little pearl.

Some of them are so uneven, that they do not preserve the figure of pearls, but the matter of them is always disposed in *strata*, like the bezoars. Now it is never questioned but that the oriental pearls are of the same nature with those which grow in other shell-fishes, as in the common oysters, and in the different sorts of muscles. All the difference between them comes only from their different water, but is every where the same matter,

matter, and the fame ftructure, as may plainly be feen in the different pearls found in the *pinna marina*. Pearls therefore muft be looked upon as true *bezoars*, as to their nature, tho' they are not perfectly fuch as to their virtue.

The pearls are not the only thing that is obfervable in the *pinna marina*. This fhell-fifh, which is a fort of great mufcle, confifts of 2 large pieces, rounded at top, and very much pointed at bottom, very unequal on the outfide, brown and fmooth on the infide, towards the point inclining to the colour of mother of pearl. They are of different fizes from 1 foot to 2½ in length, being in the broadeft part about ⅓ of their length, thefe fhells are fo thin, that they are tranfparent; what is moft remarkable in them, is a fort of tuft, about 6 inches long, more or lefs according to the fize of the fhell. This tuft is fituated toward the point of the fide oppofite to the hinge; it is compofed of feveral filaments of a very fine brown filk, thefe little threads being viewed thro' a microfcope appear hollow, when burnt they afford an urinous fmell like filk. The ancients have called this fubftance *byffus*, either on account of its refemblance to the *byffus*, of which they wove precious ftuffs, or perhaps it might be the very *byffus* itfelf, which they ufed for that purpofe; for the moft able criticks have not fufficiently cleared up what we are to underftand by the *byffus* of the ancients; they have only diftinguifhed two forts of it, that of *Greece*, which was found only in the province of *Elis*, and that of *Judea*, which was the fineft. The fcripture *informs us, that this was ufed in the facerdotal ornaments, and the wicked rich man was cloathed with it. But as the ancients under the name of *byffus*, have confounded cottons, and

* In our tranflation of the bible *byffus* is rendered *fine linnen*.

all

all other ftuffs that were more precious than wool-
len, it is not eafy to fay exactly what their *byffus*
was, and whether it was obtained only from the
fhells juft mentioned ; this is certain, that *Ariftotle*,
who calls the filk of thefe fhells *byffus*, fays it
may be wowen, and therefore it can hardly be
queftioned but that it was ufed for the cloaths of
great perfons, in the ages when filk was but little
known, and rarely feen ; in fhort this *byffus*, tho'
coarfely fpun, appears much finer than wool, and
approaches pretty near to filk : they now make
ftockings of it, and other works, which would be
more valuable, if filk was lefs common : to fpin
this fort of *byffus*, they leave it in a cellar, to grow
moift and foft ; and then they comb it, to feparate
the flocks and impurities that ftick to it, after
which they fpin it, as they do filk.

The fifhes, which afford the *byffus*, make ufe
of it to faften their fhells to the neighbouring
bodies, for as they are planted directly upon the
point of their fhells, they have need of thefe fila-
ments, which they extend all around, like the cor-
dage of a maft, to keep themfelves in that fi-
tuation.

It is probable that the *pinna marina* forms
thefe forts of threads, with the fame mechanifm
that M. *de Reaumur* has obferved in the fea-
mufcle ; but thofe of the *pinna* are more fine and
filky ; and according to *Rondeletius*, they are as
different from the threads of the mufcles, as filk
is from hemp, which may be feen by comparing
them together.

There are fome little crabs that get into the
fhells of the *pinna*, of which the antients have re-
lated fuch fingular facts, that it may not be amifs
to examine them here.

They

They thought this little animal grew with the fish of the *pinna*, for its prefervation, and therefore called it the *guardian of the pinna*, imagining that the fish perished as foon as it come to lofe its guardian. And this is what they thought made the little crab fo ufeful to its hoft.

As the *pinna* is without eyes, and befides is not endued with a very exquifite fenfation, whilft its fhells are open, and the little fifhes enter, the crab gives it notice by a flight motion, that by contracting its fhells at once, the fifhes may be taken, and then the *pinna* and the crab divide the booty between them. Thofe who did not believe that the crab received its birth in the fhells of the *pinna*, raife the wifdom of this little animal ftill higher, which in order to lodge itfelf in the fhells of fifhes, takes its opportunity when they are open, and has the cunning to put in a little ftone to hinder them from fhutting, and fo eats up the fifh. But all thefe circumftances are like a great many others related by the ancient naturalifts, without much foundation; and this is what has contributed to decry their works, tho' they otherwife inform us of feveral things that are true and curious. What they tells us here of the little crabs that lodge between the fhells of the *pinna*, is eafily difproved; for in the firft place, thefe little animals are found indifferently in all bivalve fhells, as oyfters and mufcles, as well as in thofe of the *pinna*, where we alfo fometimes find fome little fhell-fifhes, which get in or faften themfelves upon them. I have a little *concha venerea*, which was found fhut up and alive in the fhell of a *pinna*. Befides the fifh of thefe fhells does not live upon flefh, any more than the mufcles and oyfters, but only upon water and mud. Thus the cunning of the little crab is of no ufe to it.

Laftly,

Lastly, the little crabs do not eat the fishes of the shells where they lodge, for we find these fishes found and whole with the little crabs that accompany them.

It is only chance therefore that throws these little animals into the shells whilst they are open, or else they retire thither for shelter, as they are very often found in the holes of sponges and stones, and in the exterior cavities of the shells.

As I have referred to the second class of bezoars the stones of the same nature which are taken from animals, I shall add those which I lately observed in the bags of the beaver, which are called *castor*. Among several that I opened, I found one which seemed bigger than the rest, and was filled with stones of different sizes. According to the common prejudice, I should have thought that these bags had been falsified and filled with stones to increase their weight; but upon examining them, I perceived that all these stones adhered, and were pretty regular in their shape. I presented some of these stones to the flame of a candle, and they burned like those taken from the gall-bladder, and had a smell of castor. These stones pretty much resemble the kernels of medlars, as those commonly do which are found in the gall-bladder. They are tender, and disposed in *strata*, which are separated by membranes dispersed through the substance of the bag, and forming the partitions of the cells. The biggest that I have found are 6 lines long, and 4 broad, and 3 in thickness. The others, which are in greater number, diminish in bigness, and the smallest are but about the size of pins heads. It is not probable that these stones have been added in the castor, considering the manner in which I have already observed them to be constructed.

The

The juice therefore contained in thefe bags muft have thickened and curdled, about the membranes, or their glands; and fo have ferved for a bafe to the formation of thefe ftones. It is obferved, that ftones are formed in all the cavities of animal bodies, and even in the glands ; and thus the name of bezoar becomes fo extenfive. I think therefore we may range thefe ftones in the number of bezoars, as well as the different forts of pearls, fince they refemble the bezoar in their ftructure and vertue. The caftor being ufed in medicine to fortify the brain, refift poifon, affuage the vapours, and drive them out by perfpiration, the ftones which are found to contain the fame principles, muft have the fame effects, and confequently the fame virtues as the bezoardic fubftances. As I treat of the caftor only with regard to the ftones, which I have obferved, I fhall not ftop here to defcribe the animal, nor the bags which contain the fubftance called caftor, feeing the anatomy of it has been already made by the academy.

I fhall only propofe my opinion on the choice of this fubftance. I agree with thofe who are acquainted with it, that fome of it may be falfified, but I believe that the difference in its fmell and confiftence comes rather from the climate, the food, and the age of the beaver, than from any counterfeiting. The moft common, and leaft efteemed caftor is that from *Canada*. It is looked upon as counterfeited, becaufe it has either no fmell at all, or a difagreeable one. I have opened feveral that were foft, and of very little fmell, and yet without any appearance of fophiftication, for the cells were neither fwoln nor torn. On the contrary they were divided by

membranes

I

membranes adhering to the covering, as we ob-
ferve in thofe which are not fufpected to be fpu-
rious. The caftor of *Dantzick* is efteemed the
beft, and yet that of the *Levant* furpaffes it.

There are fome beavers alfo found in *France*,
in fome parts of the *Rhone*; they dry their bags,
and this fort is very good. I have fome in my
collection, dried by an apothecary of *Villeneuve-
lès-Avignon*, which are very good and large with-
out being adulterated. I have found that this
caftor was not at all inferior to that of *Dantzick*.
That of the *Rhone* is commonly fold for that of
*Dantzick*, there being no other difference between
them, but in that of *Dantzick* having the ftrongeft
fmell. I am perfuaded, that our caftor of the
*Rhone*, has the fame quality with that of the *Levant*
and of *Dantzick*; the bags are dried in the chimney
where the liquor may ferment as it dries, which
caufes the caftor to acquire continually a ftron-
ger and more proper fmell.

VI. *A machine to difengage the horfes abfo-
lutely, and at once from a coach, when they
are headftrong and run away; by M. de la
Hire, the fon*.*

The accidents, which happen when horfes run a-
way, are fo great, that I thought I fhould do a good
piece of fervice to the publick, in finding out fome
eafy way of hindering thefe accidents.

Among all the machines I have thought of for this
purpofe, I have found none more fimple than that
I am going to defcribe; after having explained
the figure which reprefents the whole fore-part of
a coach, called the *fore-carriage*, in order to fhew

* *Nov.* 16, 1712

what is called the *fplinter-bar*, with all the parts that depend upon it, the place where it is faftened, and its ufe.

*Fig* 13. AB is the pole, of which the fartheft extremity from the coach, which is furnifhed with a hook QR, is reprefented underneath.

CD, CD the two futchells.

EF the fplinter-bar faftened upon the two fut-chells, with 2 iron fcrews.

NN are the cramps of the fplinter-bar, which go into it, and are faftened at the mouldings, they hinder the 2 leather rings GH, GH, which pafs within as in the 2 cramps PP of the bars, from getting out at the end of the fplinter-bar.

LK, LK are the two bars fufpended, as we fee, at the two ends of the fplinter-bar, by means of the two leather rings, and in the middle of the bars are the cramps PP, which hinder them from flipping in the leather rings.

LM are the traces with which the horfes draw the coach; they are fitted at each end of the bars in fuch a manner, as to embrace them the more ftrongly, in proportion as the horfes draw with more effort.

S, the foot-board.

*Fig.* 14. reprefents the end of a fplinter-bar more large, with the bar which is faftened to it by the leather ring, to fhew all the fmall parts of it better.

I believe this explanation of the fore-carriage will be fufficient to underftand, what I fhall fay in the fequel of this memoir.

The machine in queftion is applied at each ex-tremity of the fplinter-bar, which is furnifhed with 2 mouldings, two or three inches diftant from each other, and raifed about ½ an inch above the

* Plate V.

body of the fplinter-bar, which is cylindrical, be-
ween thefe two mouldings.

It is between thefe two mouldings that the lea-
ther ring of the bar is placed ; it is made of feveral
leather thongs fewed together, the two ends of
which are joined by a buckle; in this manner the
leather ring is drawn into length, and at one of its
extremities embraces the end of the fplinter-bar,
and the other at the middle of the bar.

*Fig.* 15. One of the ends of the fplinter-bar is
reprefented by the letters A A ; of the two mould-
ings, which are there, that which is moft at the
extremity of the fplinter-bar has been fhaped to
8 angles, that the end of the fplinter-bar may be
the lefs diminifhed, becaufe of the fubjection which
I am going to mention ; there are 4 of them large
and 4 fmall, to give a greater feat to the fides of
the ferril, which fhall be fitted to it ; the figure
that has been given to this moulding muft be in-
fcribed in the end of the fplinter-bar, which bears
the leather rings of the bar, that it may enter into
the iron ring O.

This moulding being thus made octangular, has
two of its great fides placed vertically, one to-
wards the coach, and the other towards the horfes;
the two others are placed horizontally, one above
and the other below; it is furnifhed with a fort of
iron ferril B, which is alfo octangular.

From the face of this ferril, which is toward the
coach, there comes out perpendicularly an iron
pin D, about an inch and $\frac{1}{4}$ long. The extremity
of this pin D is rounded like a pivot, that it may
enter into a round hole at the extremity L, of the
piece of iron LK, which I call the traverfe, and
be rivetted in fuch a manner into it, that this tra-
verfe, which is perpendicular to the pin D, may
move circularly in it.

The.

The traverse LK about 4 inches long, and 10 lines broad in the place where it is moft fo, has toward the extremity K a hole M, which pierces thro', to let the bit of iron pafs when it is preffed upon it.

The piece of iron F, which is an inch and $\frac{1}{2}$ high, is raifed perpendicularly upon a pin E, which being parallel to that which is mark'd D, rifes from the middle of one of the vertical faces of the fort of ferril C, namely from that which is toward the coach ; the moulding to which this ferril belongs, has been made quadrangular ; fo that two of its faces are vertical, and two horizontal, and the ferril C is circumfcribed to the part of the fplinter-bar, between the 2 mouldings.

Acrofs the two upper horizontal faces B and C of the two ferrils, as well as of the oppofite faces, and acrofs the fplinter-bar, pafs two fkrews, which have their heads marked 3 and 4, and enter into the lower horizontal faces, which are oppofite to B and C, to fix the two ferrils B and C to the fplinter-bar AA.

The thong PQRSTVXY is the leather ring of the bar, it embraces at one of its ends that part of the fplinter-bar, which is between the two ferrils B and C, becaufe the extremity PQ is fewed to the under part; it is flit in the middle of the fpace between the two ferrils, to receive in its thicknefs the iron ring O ; this ring embraces the fplinter-bar in this place, and has a pin or tongue 8, which is perpendicular to it, and enters into the hole Z, marked toward the extremity XY of the leather ring of the bar.

When the tongue 8 is entered into the hole Z of the leather ring, if you pafs the bar into the leather ring at the place TV, you will make the ring O turn, and the tongue 8 will come before;

it

it is to hinder it from coming before, that we shall lower the traverse LK upon the pin E, and make the double key H enter into the hole G, which goes thro' the piece F; but as the dirt might fall between the plates of the key H, and hinder it from being drawn back, there is faftened upon the traverse LK the box N, the entrance of which will be fhut by the head of the key when it is put in, fo that nothing can get into the box.

We fee by the conftruction of the machine, that the horfes which draw the bars, make a continual effort to raife with the tongue 8 the traverfe LK, which cannot get loofe from the piece F, becaufe of the key H which paffes through it.

The other bar is faftened in like manner, at the other end of the fplinter-bar.

There remains nothing now but to fhew how we may draw all at once, in the fame inftant, the key H, and that which is at the other end of the fplinter-bar.

To do this we take a leather ftrap or thong flit in one part of its length, one of the ends of which paffes over one pulley, and the other over another; thefe two pulleys are placed horizontally, and are in a double block, the gudgeon of which enters perpendicularly into the middle of the length of the fplinter-bar, and into the face which is toward the coach.

The two ends I of this ftrap, after having paffed over the pulleys, embrace the rings 6, which are behind the heads of the keys, and there thefe two ends, or thefe two parts of the flit end of the ftrap are fewed together; the other end of the ftrap, which has not been flit, paffes under the foot-board and thro' the axle-tree of the little wheels, and is faftened to a filk cord, which enters the coach at a

hole

hole made in the middle of the fore-part, like that thro' which the ftring paffes, to give the coachman notice when to ftop.

It is eafy to fee, that if the perfon in the coach pulls the ftring, whilft the coach is going on, he will difengage the pins H from the holes G, that at the fame time the tongues P will raife the traverfes LK ; that the end of the leather ring XY will come off from the tongue 8, and confequently that the bars will be no longer faftened to the fplinter-bar ; and as the bars are no longer faftened, the coach can no longer be drawn by the horfes, becaufe they are only faftened by this part ; having fubftituted a fpring inftead of the little ftrap, to hold the great leather rings engaged in the iron hook, placed at the end of the pole ; thefe leather rings lower this fpring, when the horfes continue to go on, and the bars are no longer faftened to the fplinter-bar, which I fhall explain more at length, when I fpeak of the other advantages of this fpring.

The convenience of this machine is, that it may very eafily be taken off from one coach and put upon another ; for you need only undo the two fkrews 3 and 4, and to unfew and few again the two ftraps which hold to the two keys, becaufe I fuppofe that care will be taken, before the two mouldings at the two ends of the fplinter-bar are made angular, as I have obferved, to faften a double pulley to the middle of the fplinter-bar, and to make a hole thro' the axle-tree of the little wheels of all the coaches, to which we would have the machine applied, and they will ferve as ufual, when the machine is not on, becaufe we may pafs into the holes of the fplinter-bar, where the two fkrews 3 and 4 enter, the pins of a cramp, which fhall be furrowed at the end, to fkrew nuts upon

them

them to hold the cramp faft to the fplinter-bar, and by this means hinder the leather rings of the bar, from flipping off the end of the fplinter-bar, as is done to all coaches.

It is not enough to fhew the way to prevent accidents, when two horfes run away, but we muft alfo prevent them, when there are 4, with a poftillion or without one, and when there are 6, and when the poftillion has been thrown under the horfe that he rode upon, or when he cannot govern the horfes.

When there are four horfes to a coach, the two before are faftened either with traces, to the wheel horfes, or to two bars faftened to a fplinter-bar by two leather rings, like thofe next the coach; this fplinter-bar, to which the two fore-horfes are faftened, is encompaffed in the middle part by a leather ring, in which their paffes another, where the end of the pole enters, which has a hole to receive an iron pin, which is faftened to the end of the hook, with a little flip of leather, and againft this pin the leather ring refts into which the pole enters; in this mannner the fore-horfes draw the coach by the end of the pole, but when inftead of four horfes they put 6, they only faften the two firft, to the two fecond with traces; and fo the four firft are faftened to the coach only by the end of the pole.

In order therefore to hinder the accidents which might happen, if four or fix horfes were to run away, we muft find a way to detach all at once the leather ring, which is faftened to the end of the pole, which the machine I am going to defcribe will do, after having faid that the wheel horfes have each of them faftened to the breaft leather of their harnefs a great long leather ring, in which

<div align="right">which</div>

which another leather ring is received, in which the end of the pole enters quite to the bottom of the hook.

*Fig.* 16. the end of the pole is reprefented by AB, and CDE reprefents the hook which is applied to it, and differs from the common ones only in that the end E is flatted, having a flit KF in its thicknefs, and pierced quite thro' with a fquare hole G.

In th's flit KF between the extremity FK of a piece of iron FKL, which turns upon a hinge in the piece of iron MNO, this piece enters perpendicularly into the pole almoft to the middle, becaufe the plate of the hook, and the pole are perforated with a hole QR rounded at the two ends, of which the breadth is equal to the diameter of the pin MNO at the bottom, and the length to the quantity with which the pin MNO enters the pole ; it is againft this pin that the ring refts, which holds the fplinter-bar to which the fore-horfes are faftened.

Therefore to detach the fore-horfes from the end of the pole, we need only faften the piece of iron LKF to the end of the hook E, fo that it may eafily undo : this may be done by means of a fpring ST, of which the end S is held faft to the plate of the hook by the fquare rivet V, and the other end T, which is of the breadth of the end of the hook E is perforated by a long flit, in which there paffes a ring H, which holds to the extremity of a fquare iron pin, which paffes thro' the 2 thickneffes of the end of the hook, and of the part FK of the piece FKL, and fills the fquare hole G.

The iron pin HG is inftead of the flip of leather which holds to the end of the hook the iron pin which paffes into the end of the pole. An-

other

other ring I is engaged in the ring H, or a little
flip is tied in a knot to it, that when the spring ST
is lowered, it may make the pin HG come out
of its hole.

I have said there were two leather rings in
which the end of the pole went quite to the bot-
tom of the hook, that is, they occupy the place
SP; but as the end of the pole cannot be made to
go into these rings without lowering the spring;
so they cannot get out without lowering it,
and also difengaging at the fame time the pin
HG, which no longer retaining the piece FKLO
in the end of the hook, it will be carried away by
the leather ring which refts againft it, and the
fore-horfes will go away with the fplinter-bar.

We fee by this conftruction, that if by the firft
machine we detach the 2 horfes from the pole,
they cannot go forward without lowering the
fpring ST, and at the fame time detaching the
fplinter-bar from the end of the pole, and confe-
quently the horfes which are faftened to it.

VII. *A comparifon of the obfervations of the
    eclipfe of the moon,* Jan. 23, 1712, *in the
    evening, made at* Nuremberg, *by M.* J. P.
    Wurfelbaur, *and at* Paris, *at the royal ob-
    fervatory by Meff.* de la Hire*.

We are not to expect fo great an exactnefs
from the comparifon of thefe obfervations, as
from thofe of the eclipfes of the fatellites of *Ju-
piter.* And yet we ought not to neglect them,
fince we may obtain from them confiderable ad-
vantages to geography, when we have not an op-
portunity of obferving thofe of the fatellites; and

* July. 30, 1712.

ef-

Pl. V. *Vol. IV.*

*Fig. 1.*

*Fig. 2.*

*Fig. 3.*

*Fig. 4.*

*Fig. 5.*

*Fig. 6.*

*Fig. 7.*

*Fig. 8.*

*Fig. 10.*

*Fig. 11.*

*Fig. 12.*

*Fig. 13.*

*Fig. 14.*

*Fig. 15.*

*Fig. 16.*
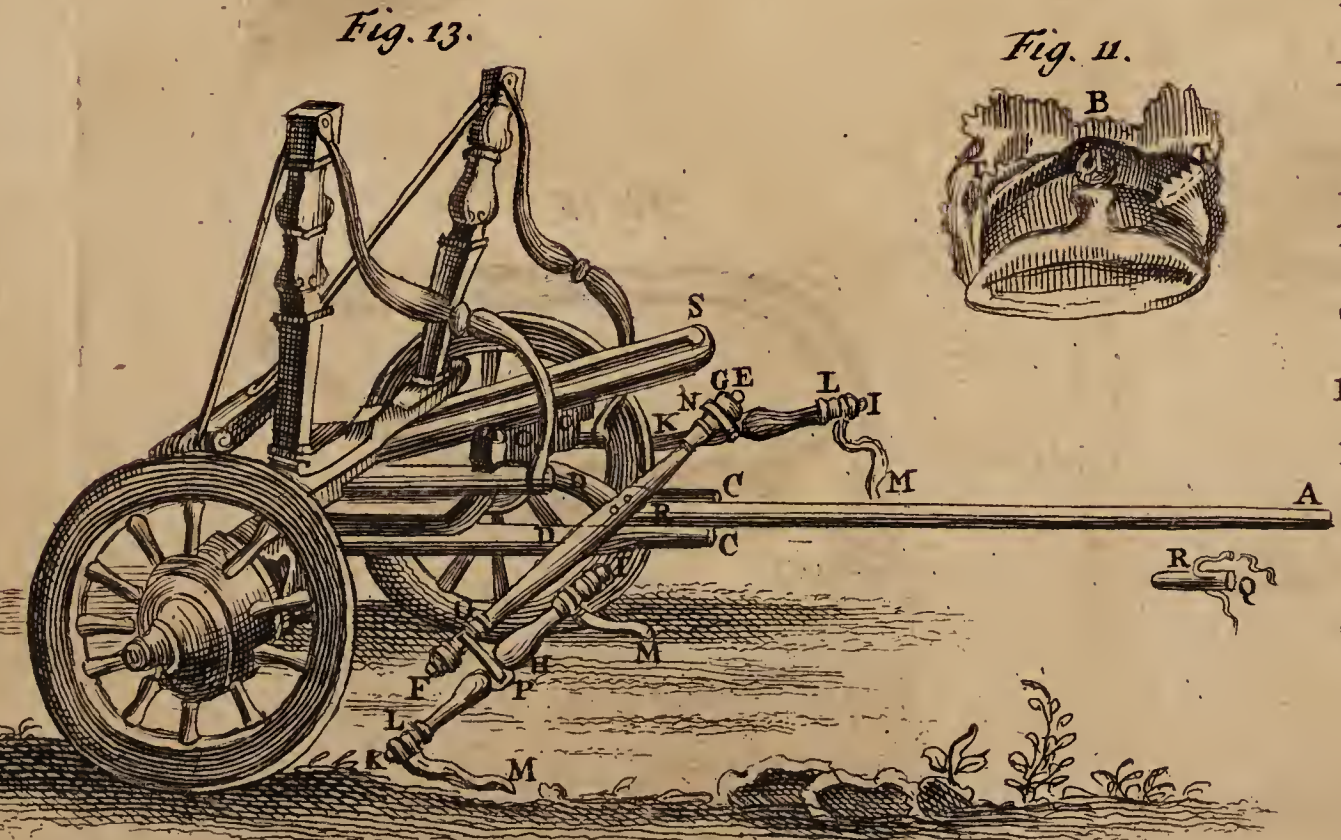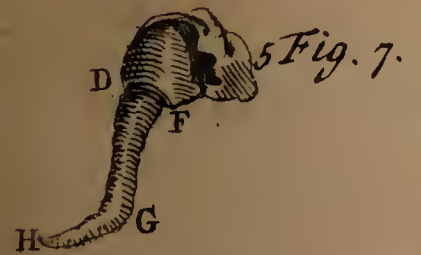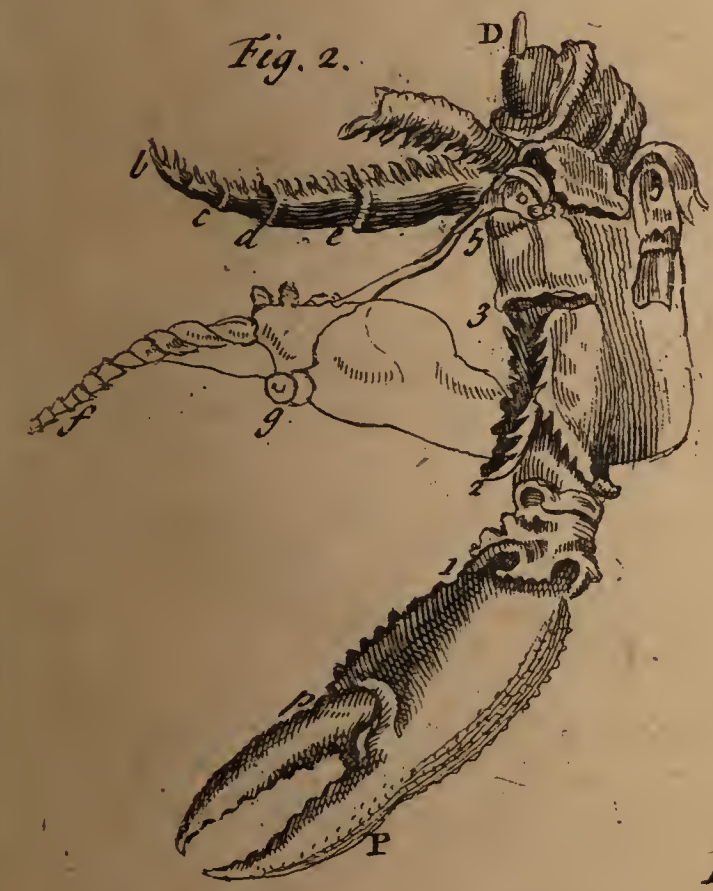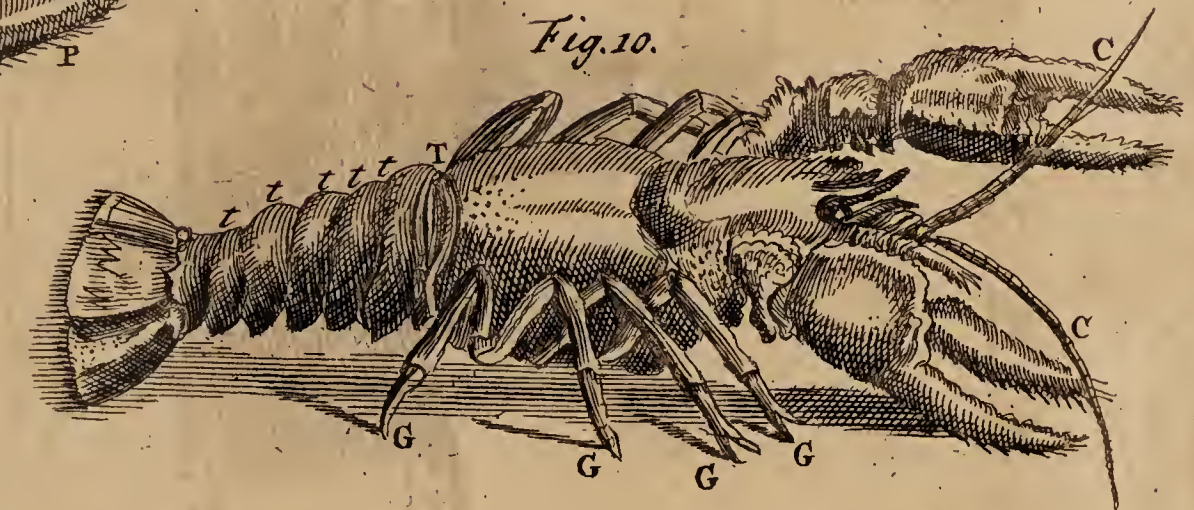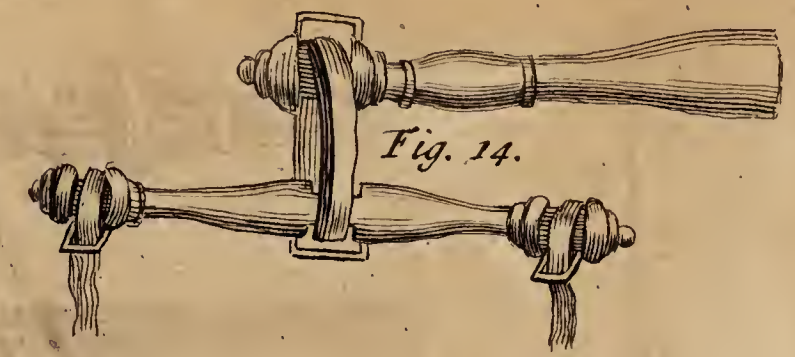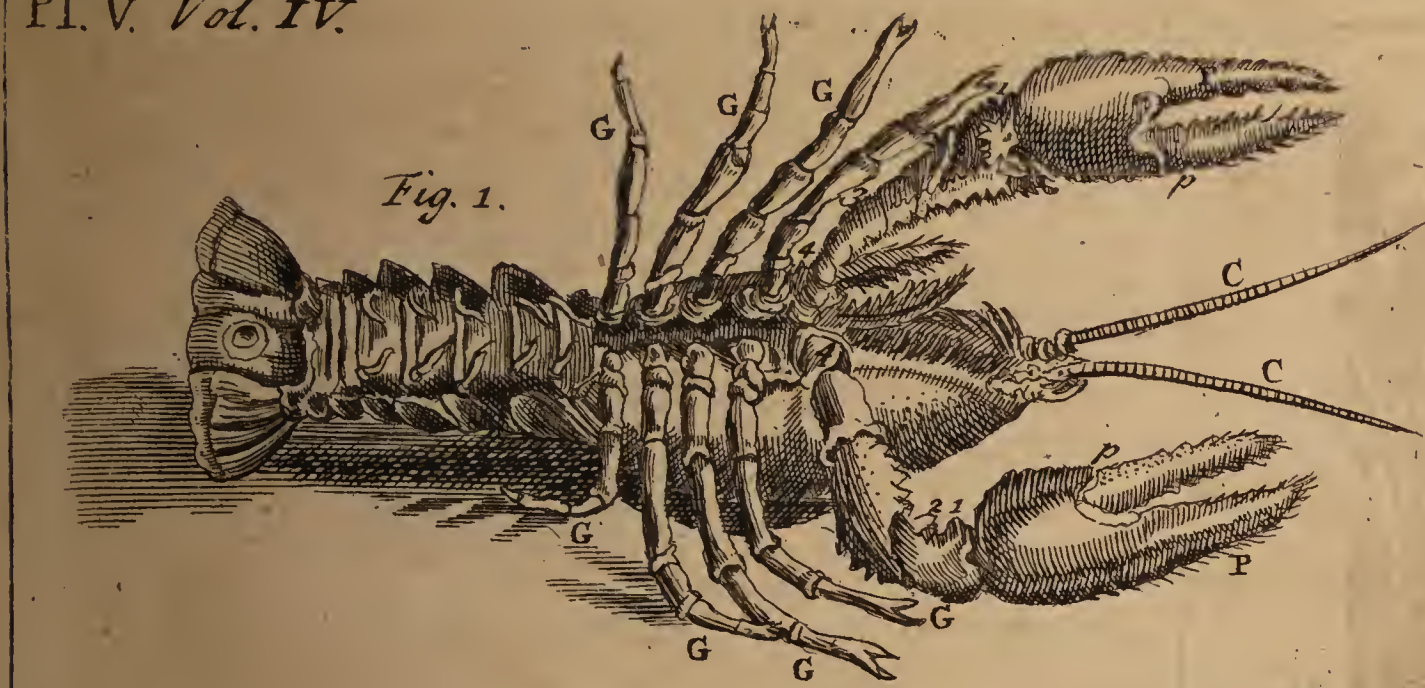
X

V

T

S

R — *26 inch.*

Q

P

O

N

M — *Fig. 12.*

L

K

I

H

G

F

E — *27 inch.*

D

C
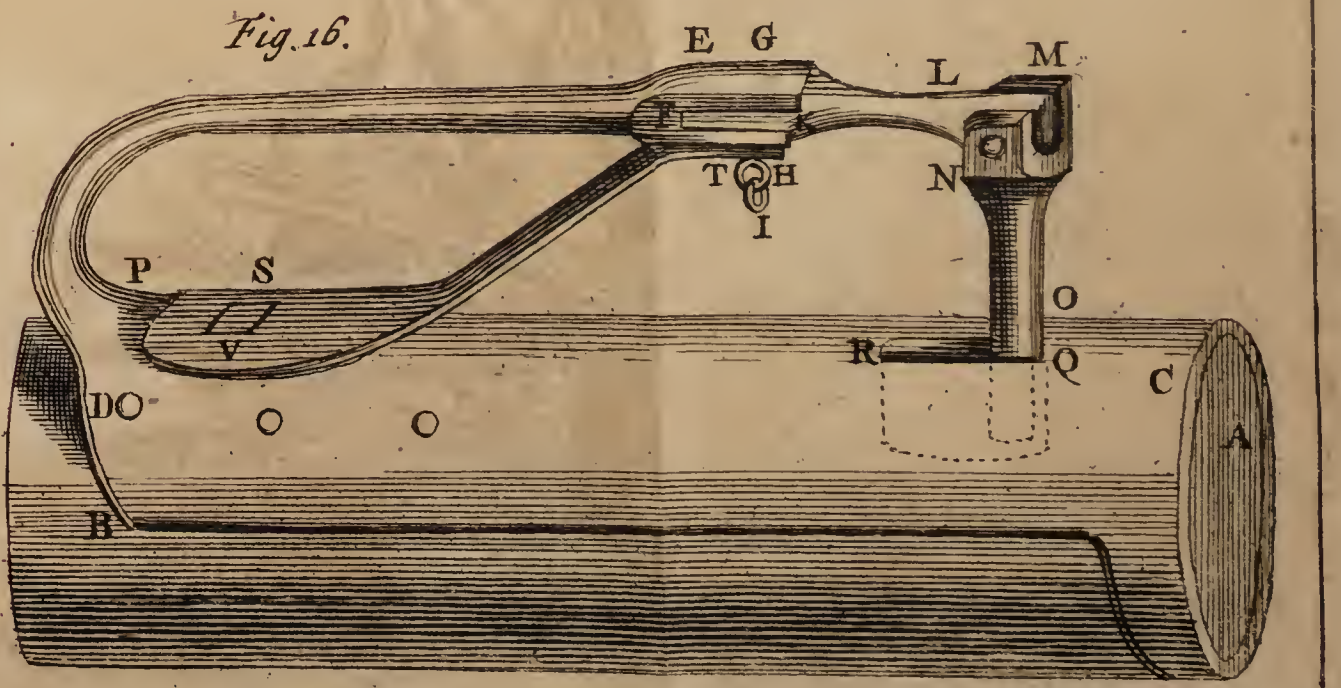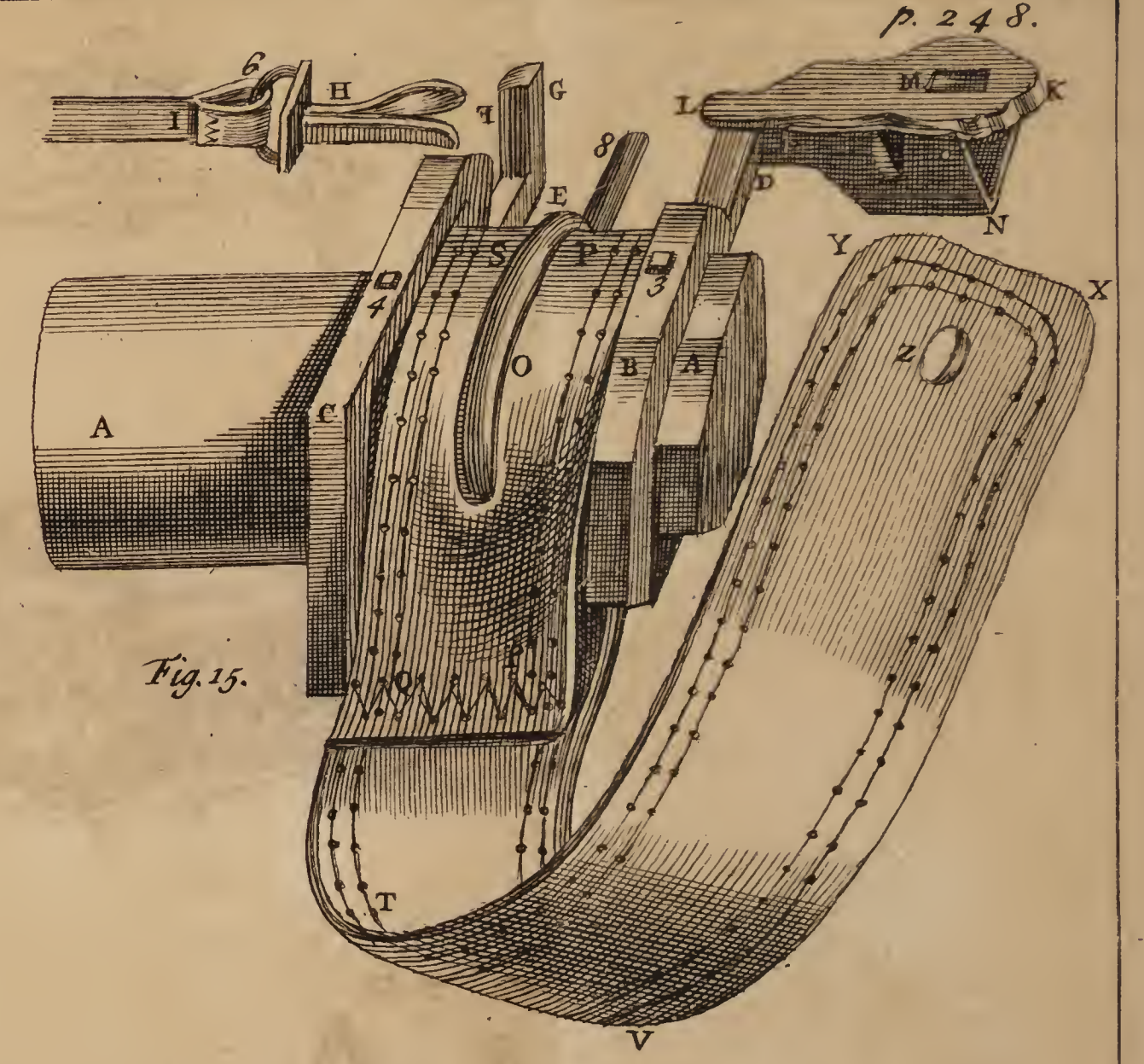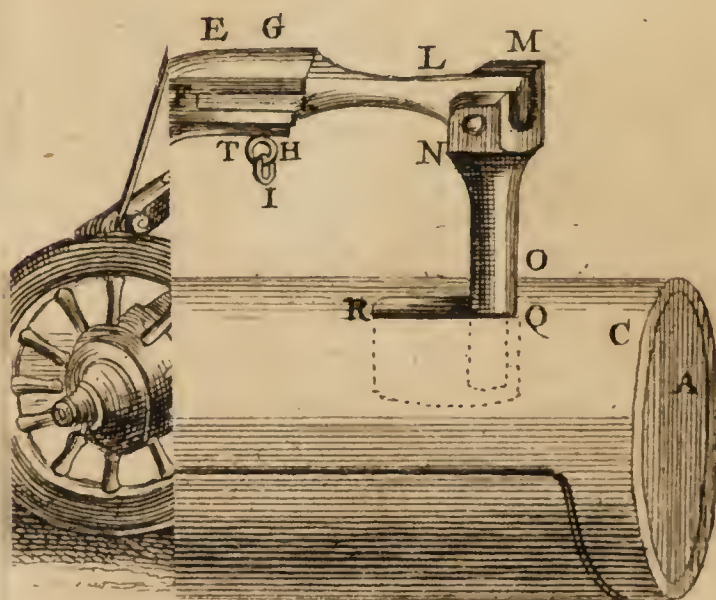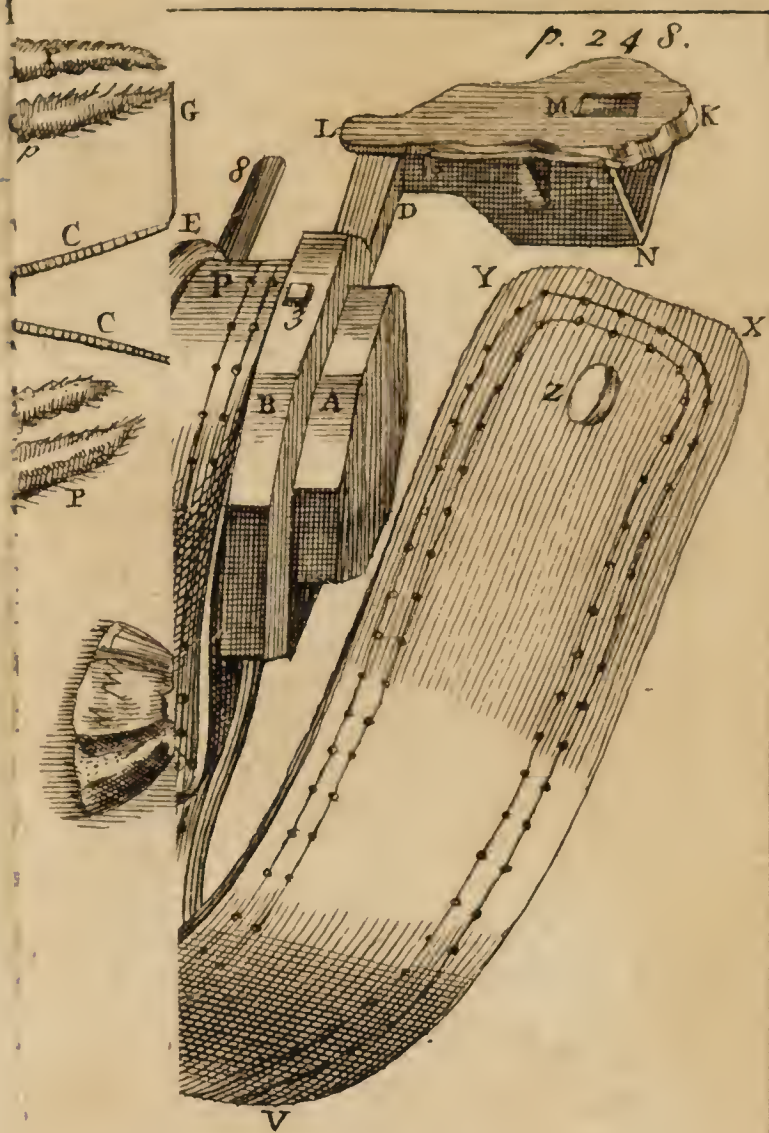
*J. Mynde sc.*

J. Mynde sc.

J. Mynde sc.

efpecially with regard to places that are very diftant from each other; and if ufe had been formerly made of thofe which were obferved, there would not have been fuch defective maps made as thofe which were publifhed before the academy began to make ufe of the fatellites of *Jupiter* for the determination of the longitudes.

We have drawn the following table from the obfervations of M. *Wurfelbaur*.

| At *Nuremberg.* | | | Phafes. | | At *Paris.* | | | Difference. | |
|---|---|---|---|---|---|---|---|---|---|
| h | $\prime$ | $\prime\prime$ | $\circ$ | $\prime$ | h | $\prime$ | $\prime\prime$ | $\prime$ | $\prime\prime$ |
| 7 | 28 | 40 | 1 | 0 | 6 | 50 | 50 | 37 | 50 |
| 7 | 32 | 19 | 1 | 30 | 6 | 58 | 50 | 33 | 29 |
| 7 | 50 | 40 | 3 | 0 | 7 | 25 | 20 | 35 | 20 |
| 8 | 53 | 35 | 2 | 20 | 8 | 17 | 35 | 36 | 0 |
| 8 | 56 | 55 | 2 | 0 | 8 | 18 | 19 | 38 | 36 |
| 9 | 2 | 51 | 1 | 30 | 8 | 30 | 20 | 32 | 31 |
| 9 | 18 | 10 | End | | 8 | 44 | 0 | 34 | 10 |

*Paffage of the fhadow thro' the fpots.*

| h | $\prime$ | $\prime\prime$ | | h | $\prime$ | $\prime\prime$ | $\prime$ | $\prime\prime$ |
|---|---|---|---|---|---|---|---|---|
| 7 | 31 | 35 | *Heraclides* | 6 | 59 | 8 | 32 | 27 |
| 7 | 38 | 10 | *Plato* | 7 | 4 | 50 | 33 | 20 |
| 8 | 49 | 10 | *Helicon* | 8 | 16 | 10 | 33 | 0 |
| 8 | 53 | 35 | *Harpalus* | 8 | 16 | 10 | 37 | 25 |
| 8 | 58 | 55 | *Plato* | 8 | 24 | 15 | 34 | 40 |

The magnitude of the eclipfe

At *Nuremberg*                    At *Paris*
3 digits, 42'.                    3 digits, 40'.

Taking a mean between thefe obfervations, we fhall have for the difference of the meridians betweeen *Nuremberg* and *Paris* 34' $\frac{1}{2}$, which is pretty nearly as I determined it in my tables.

# A
# TABLE

## OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCXIII.

## In the HISTORY.

## In the MEMOIRS.

VII.

Kk 2                    A N

# AN
# ABRIDGMENT

## OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the year 1713.

## I. *Experiments on sea-coal.*

M. *Deflandes* being in *England*, made with the sea-coal that is burned there, two experiments which he believes have escaped the *English*.

*Exp.* 1. Having beaten some coal, he put about half an ounce of it into a glass of water, which became quite black. He left the glass exposed to the air all night upon the window, it was in winter, and the next day he found that the water which was frozen, was of a reddish colour. To give this colour to the water, the frost must have disengaged the sulphurs of the coal, though this action does not seem very well to agree with it.

*Exp.* 2. Some of the ashes of this coal infused in brandy, and mixed with steel filings, makes a black tincture, which clears as it heats. When it begins to boil, it takes a softer colour than the common iron-grey. M. *Deflandes* dyed some wool with this agreeable tincture, and no artist could imitate it.

II.

II. *The history of the* carcajou, *an American animal.*

M. *Sarrasin*, the king's physician at *Canada*, and a correspondent of the academy, from whom we have seen a very curious and exact history of the *castor* or beaver in the memoirs of 1704*, has also sent such another of the *carcajou*, of which we here give an abridgment.

The *carcajou* is a carnivorous animal, which inhabits the coldest parts of *North-America*. It commonly weighs from 25 to 35 lb. It is about 2 feet from the end of the snout to the tail which is about 8 inches long. Its head is very short, and thick, in proportion to the rest of its body, the eyes very small, the jaws very strong, and furnished with 32 sharp teeth ; notwithstanding it is small, it is very strong and furious, and tho' carnivorous, it is so slow, and so heavy, that it crawls upon the snow rather than walks upon it.

As it walks, it can catch no other animal than the beaver which is as slow as its self, and that must be in summer when the beaver is out of its cabbin. But in the winter it can only break and destroy the cabbin, and surprize the beaver, which but very seldom succeeds, because the beaver has its sure retreat under the ice. However, as the beaver, even in winter goes into the woods to seek for fresh provisions, which he likes better than stale, the carcajou may attack him there.

The chase which is most successful to him is that of the elk, and *Caribou*, or *Canada* stag. The elk chuses in winter a place where there grows a quantity of *Anagyris fœtida*, or stinking bean

* Vol. II. pag. 181. of this abridgment.

tre-

ᵗrefoil, becaufe it feeds upon it, and when the ground is covered 5 or 6 feet with fnow, he makes roads in thefe places which he never quits, unlefs he is purfued by the hunters. The *carcajou* having obferved the elk's road, climbs up into a tree near which he muft pafs, and from thence leaps upon him, and cuts his throat in a moment. In vain does the elk lie upon the ground, or rub himfelf againft the trees, for nothing will make the carcajou let go his hold, and the hunters have fometimes found pieces of his fkin as large as one's hand, which have ftuck to the tree againft which the elk had rubbed himfelf.

The *caribou* is a fort of ftag. It is very light, and runs upon the fnow almoft as faft as upon the ground, becaufe his nails which are very broad, and furnifhed with rough hairs in their intervals, hinder him from finking, and ferve him inftead of the *raquette* † of the favages. When it inhabits the thick woods, it makes roads in winter like the elk, and is in the fame manner attacked there by the *carcajou*. But when it is in open places, where it has not need of making roads, and where it goes indifferently on all fides, the *carcajou* which might wait too long without fuccefs, is not accuftomed to loofe his time, and does not chafe the *caribou* but in thick places ; fo ingenious is his ardor for his prey.

III. *An obfervation on the areometer.*

As F. *Feuillee* a minim, correfpondent of the academy, read to it the relation of a voyage which he had juft made into *South-America*, and

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† A fort of broad fhoes which the favages wear to keep their feet from finking into the fnow.

as he fpoke of the obfervations which he had made with the areometer with the weight of the fea-water in different places, it was objected to him, that in a warmer climate the glafs of the areometer muft dilate, and confequently occupies more fpace in the water that is weighed, and makes it appear lefs heavy than it really is. But M. *Caffini* anfwered, that the water itfelf was alfo more dilated by the heat, and actually weighed lefs.

A N

# A N
# ABRIDGMENT
### OF THE

PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1713.

I. *Meteorological observations made at the royal observatory, by M.* de la Hire*.

THESE are the observations of the quantity of rain-water, and melted-snow, with the changes of the weather marked by the thermometer and barometer during all the last year 1712. All these observations have been made as in the preceding years, in the same place, and with the same instrument. The height of water fallen in.

| | Lin. | | Lin. |
|---|---|---|---|
| Jan. | 20 $\frac{1}{8}$ | July | 36 $\frac{1}{2}$ |
| Feb. | 8 $\frac{1}{2}$ $\frac{1}{4}$ | Aug. | 6 |
| March | 6 $\frac{1}{4}$ | Sept. | 39 $\frac{1}{4}$ $\frac{1}{8}$ |
| April | 51 $\frac{1}{8}$ | Oct. | 25 $\frac{1}{2}$ $\frac{1}{4}$ |
| May | 12 $\frac{1}{4}$ $\frac{1}{8}$ | Novem. | 16 $\frac{1}{4}$ |
| June | 23 $\frac{1}{8}$ | Dec. | 8 $\frac{1}{2}$ $\frac{1}{8}$ |

The sum of the height of water for the whole year, 254 lines $\frac{1}{4}$, or 21 inches, 2 lines $\frac{1}{4}$, which is more than the mean years, which we have determined at 19 inches.

My thermometer was at the lowest the last day of the year, and marked 24 $\frac{3}{4}$ of its parts pretty near the same as the 8th of *Jan.* which

* Jan. 7, 1713.

shews,

shews, that the cold has not been great, for it often falls to 14, and in the mean state it is at 48, as in the bottom of the caves of the observatory, where it always remains at the same point.

This thermometer was at the highest at 64 parts the 16th of *August*; but as that was at sun-rising, the time when I always make these observations, and as in the greatest heat of the day, which is towards 2 in the afternoon, it rose above the state it was at in the morning 12 parts, it must be considered as 76 for the greatest heat, and consequently the difference marked between the greatest cold, and the greatest heat will be 52 parts, the half of which is 26, which being added to 24, makes 50, which is not far from 48; by which we know, that the cold has been very near as much below the mean state, as the heat has been above it.

My common barometer was at the highest at 28 inches, 4 lines, $\frac{2}{3}$, the 10th of *Feb.* and about that time it was always very high. The heavens were then pretty serene, and very little north winds; and I observed also, that whenever this barometer was higher than 28 inches, which happened pretty often in the year, the wind was towards the north and east, and sometimes with fogs. I have another barometer where the quicksilver is always 3 lines higher than in that with which I commonly observe. This common barometer was at the lowest once only, the 6th of *Nov.* at 26 inches, 10 lines, $\frac{2}{3}$, the sky being serene with a moderate east-wind; but the quicksilver presently rose again, and the wind turned toward the west and south-west; the difference between the greatest and least height of this barometer, was 1 inch, 6 lines, as usual.

There was nothing remarkable in the winds of this year; but I obferved in general, that in this country, every time the weft and fouth-weft wind prevailed for fome time, the heavens were cloudy toward the evening, and at the beginning of the night; and that toward the morning it was ferene. I think the reafon of it is plain enough; for in the afternoon, the fun falling almoft perpendicularly upon the feas, which are to the weft of us, raifes a great many vapours from them, which are afterwards brought to us about the beginning of the night; on the contrary, there arife but few vapours from thefe feas in the night time, and the wind continuing the fame, the fky muft be pretty clear toward the morning.

### Remarks.

It generally happens that thofe which have been wounded in any part of the body feel pains there every time the weather is difpofed to change. I think it may be accounted for in this manner: the texture of the affected part muft be very fine, infomuch that it cannot be touched without feeling of pain; and in the change of the weather, the air becoming either lighter or heavier, makes an extraordinary impreffion upon thefe parts, either by compreffing or extending them, as if they were touched by it, which may caufe the pain which is there felt.

### Of the declination of the needle.

We found the declination of the needle to be 11 degrees, 15 minutes, *Dec.* 30. This obfervation was made with the needle of 8 inches long, which we commonly ufe, and in the fame place as in the preceding years, which is a thick pillar placed at the end of the terrafs of the obfervatory toward

the

the fouth. The fide of the box of the compafs is applied againft one of the faces of this pillar ; and it has been for a long time confirmed, that this face was exactly turned toward the weft, by putting a thick rule againft it which bears two fights at its extremities, to fee if the rays of the fun, which pafs through thefe fights, agree with the true noon marked by the great pendulum clocks regulated by the fun, which was found very juft ; for this building had been placed with a great deal of care and precaution, by the late M. *Picard*.

We obferved alfo at the fame time, the declination of the loadftone with another needle of only 4 inches long, and found it the fame with that of 8 inches.

II. *Reflections on fome obfervations of the flux and reflux of the fea, made at the port of* Breft, *in the year* 1712 ; *by M.* Caffini\*.

Since the royal academy of fciences has undertaken to examine the phænomena of the flux and reflux of the fea, we have received a great number of obfervations made in feveral parts of *France*, which have ferved to find new rules, as well to eftablifh the time of the tides in each of thefe ports, as to determine their different heights : the greateft part of thefe obfervations feem to prove that there is a great relation between the motions of the moon, and thofe of the tides, fince not only the great and fmall tides follow pretty exactly the feveral phafes of the moon, but even the different heights which are there obferved are

\* Feb. 1. 1713.

in

ⁱn proportion to the feveral diftances of the moon from the earth.

But as we might fufpect that thefe various effects had been produced by fome unknown caufe, which had concurred at the fame time with the motions of the moon by a fort of chance ; twas neceffary to be affured of it by a greater number of obfervations.

We have had the opportunity of making them by a new journal of obfervations of the flux and reflux of the fea made at *Breft*, which begins the 1ft of *February* 1712, where the firft ended, and has been continued to the 12th of *July* of the fame year.

In this interval of time there have been fix and five full moons, the tides of which ha⸱ obferved; that which happened the ea⸱ obferved the 6th of *February* at 3ʰ 9′ in the ⸱ ing ; and that which happened the lateſt was ob⸱ ferved *April* the 6th at 4ʰ 12″ ½ PM. with an hour, and three minutes difference from one to the other.

However if we fuppofe the mean time of the high tide at *Breft* in the new and full moons to be 3ʰ 45′. the fame that was determined in the preceding memoir, and make ufe of the common equation of two minutes, for each hour, that the mean time of the high water is fafter or flower, with regard to that of the new or full moon, we fhall find that the high tide ought to have happened *February* 6th, the day of the greateft acceleration at 3ʰ 7′ ½ in the morning, within about 1 ½ minute of what was obferved; and that *April* the 6th, the day of the greateft retardation, the high tide ought to have happened at 4ʰ 15′ within about 2 ½ minutes of what was obferved.

The other obfervations which are to the number of 15, agree for the moft part to the calcu-
lation

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

Fig. 7.

Fig. 8.

Fig. 9.

Fig. 10.

Fig. 11.

Fig. 12.

Fig. 13.

J. Mynde Sc.

ig. 11.   Fig. 12.

I

Fig. 1

T

P

N R

Fig. 3

G

F

E

D

C

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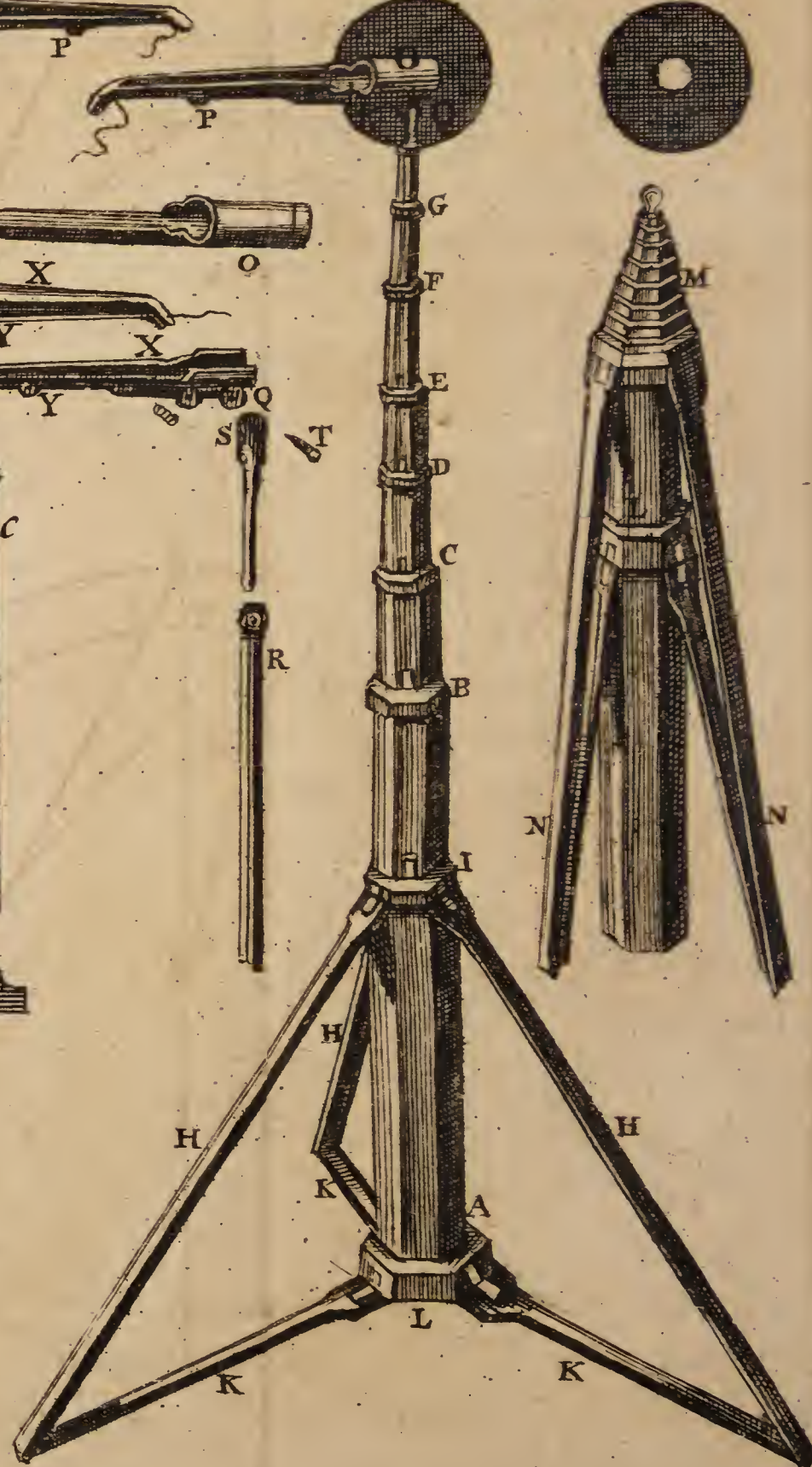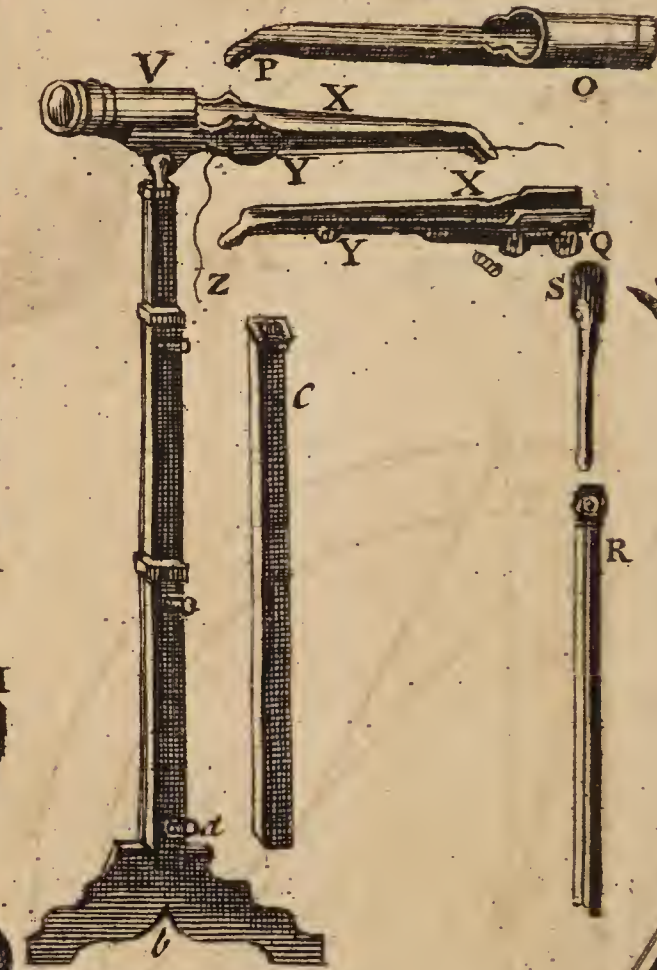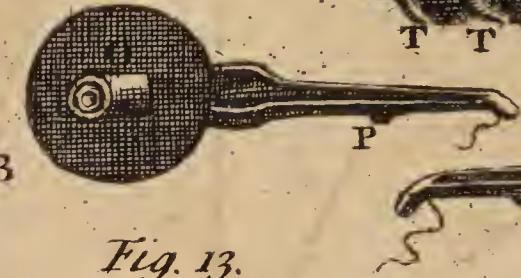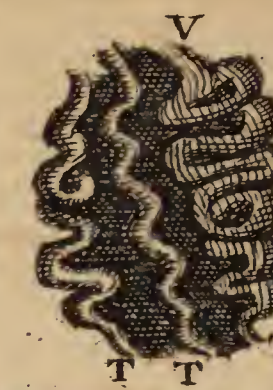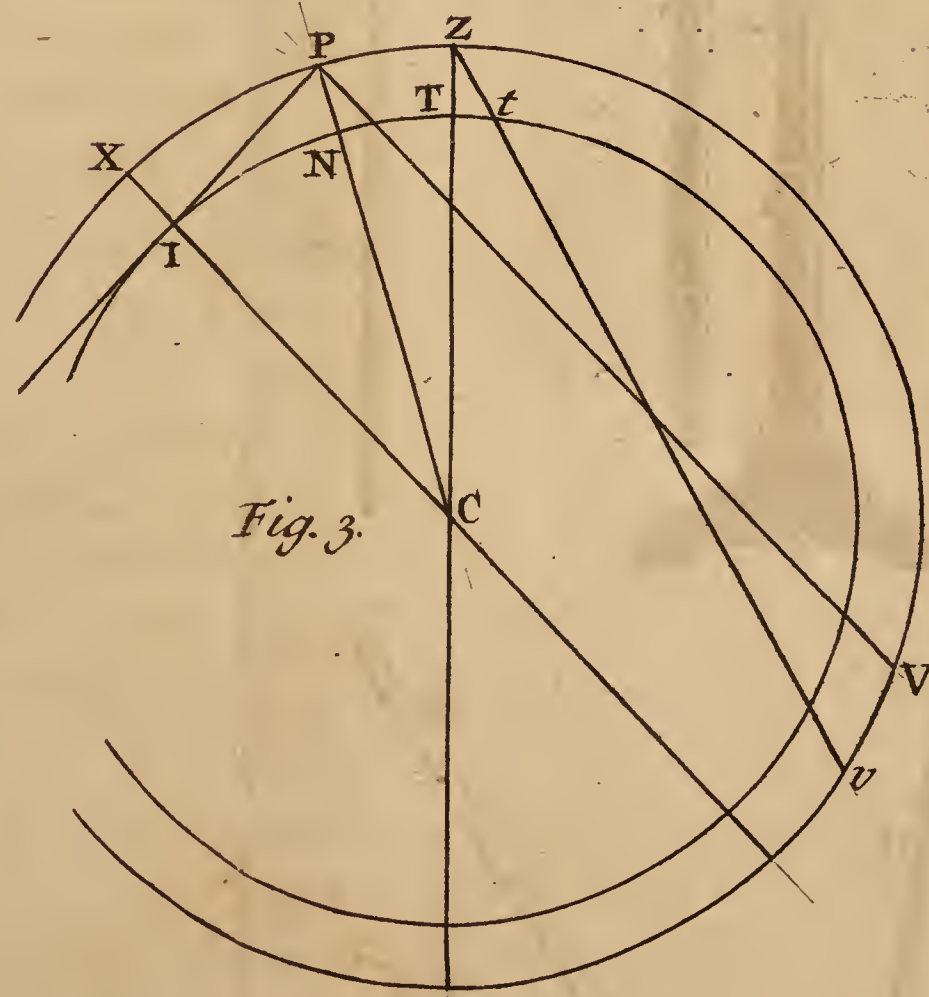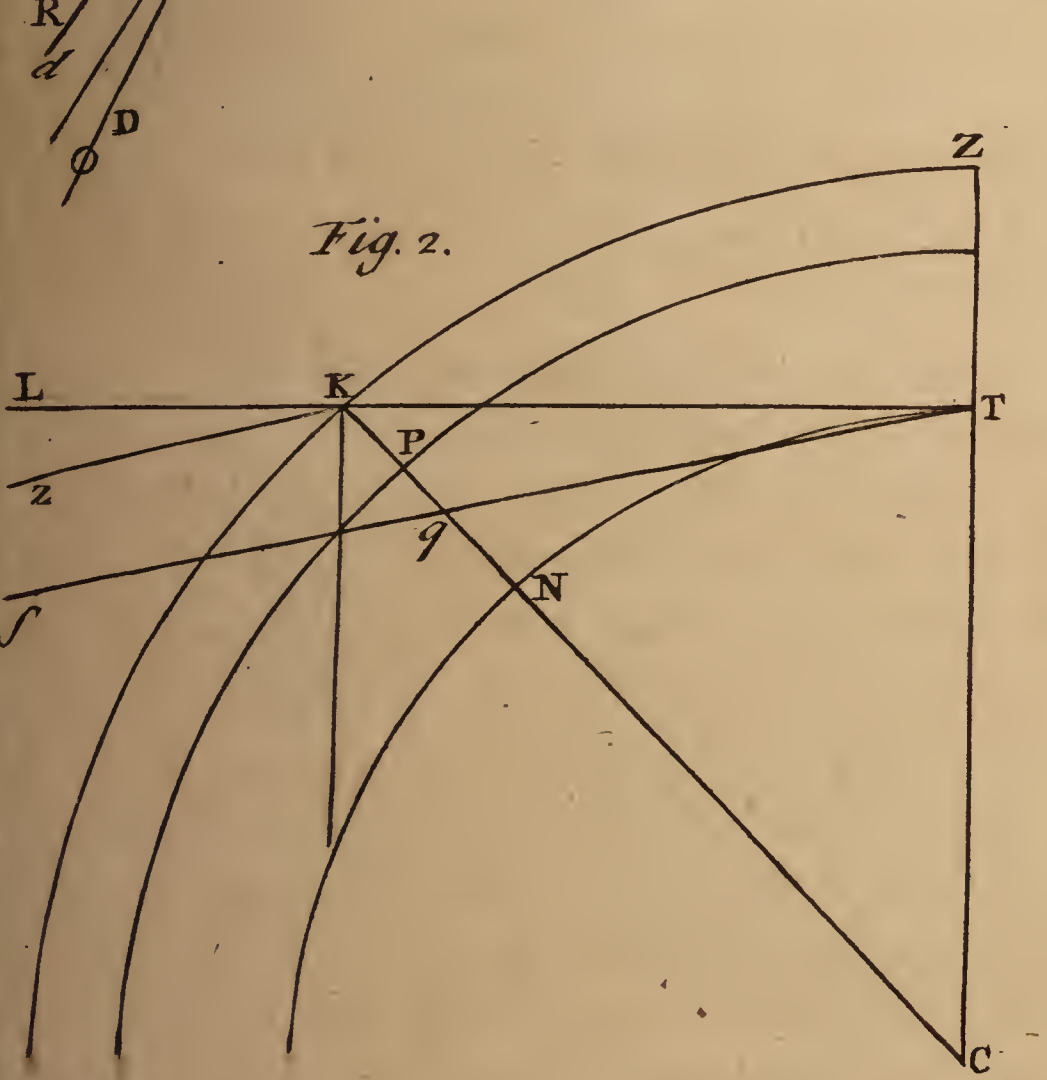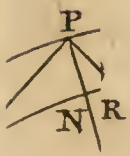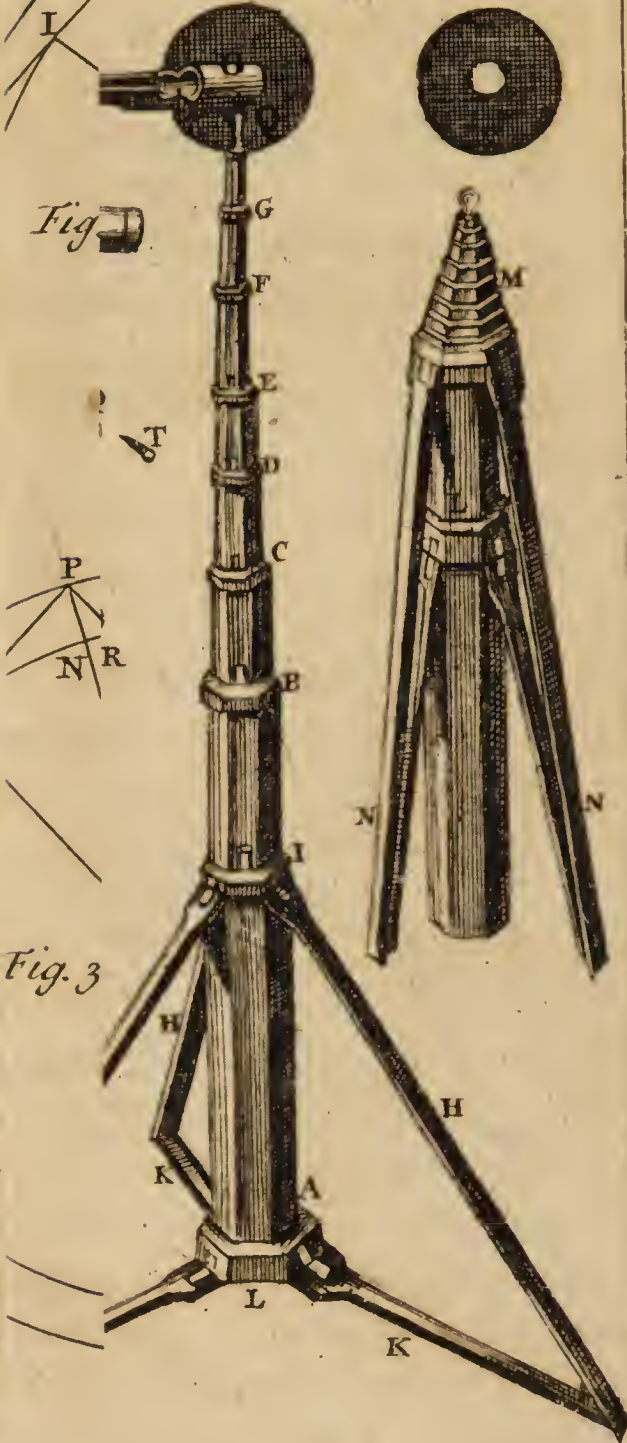
lation within a few minutes, and the fartheft dif-
tant are only 14 minutes from it, which is an ex-
actnefs to which we could not hope to have arrived
if we confider that it is often difficult to be af-
fured within a quarter, or even almoft half an hour
of the high-water.

With regard to the time of low-water, obferved
in the fame phafes of the moon, we find that it
agrees alfo pretty exactly to the calculation, with
this difference only, that the fea takes up fome
minutes more in falling than in rifing, as we have
already obferved. This difference may rife at
Breft to about a quarter of an hour in the new and
full moons, and to half an hour in the quadratures;
and this rule is fo generally obferved, that of
all the obfervations that we have examined for a-
bove the fpace of a year, there are only four or
five which are not conformable to it.

As to the time of the high-water obferved in the
quadratures, it is fubject to more irregularites than
in the new and full moons.

The high-water which happened the earlieft,
was obferved the 29th of *March* at $8^h$ $8'$ in the
morning, and that which happened the lateft was
obferved the 28th of *April* at $10^h$ $11'$ at night,
with a difference from one to the other of $2^h$
$3'$. But thefe differences are partly corrected, by
fuppofing the mean time of the high water at
*Breft* in the quadratures at $8^h$ $57'$, the fame that
was determined in the preceding memoir, and
making ufe of the common equation of $2\frac{1}{2}$ mi-
nutes for each hour, that the mean time of the
high water is fafter or flower with regard to the
time of the quadratures. For we fhall find that
the high tide ought to have happened the 29th of
*March*, the day of the greateft acceleration at $8^h$
$34'$ in the morning, within about 26 minutes of
what

what was obferved, and that the 28th of *April*, the day of the greateft retardation, the high tide ought to have happened at $9^h$ $46'$ at night, within about 25 minutes of the obfervation.

We have obferved in the preceding memoirs, that the retardation of the tides is greater toward the quadratures, than toward the new and full moons. This is conformable to our obfervations, and feems furprifing when we confider, that in the new and full moons, the fea rifes fometimes at *Breft*, to the height of 21 feet, whereas in the quadratures it fometimes rifes only 4 or five feet, and feldom to 11 feet.

However if we fuppofe that the motion of the tides is made by a fort of impulfe, as we have great reafon to imagine; we find that experience may agree with reafon; for the preffure of the air upon the fea, may be fuch as not only to make the fea rife to a greater degree of height in the new and full moons than in the quadratures, but alfo to make it arrive to this greater height with more velocity.

With regard to the time of the greateft tides in each lunation, we find that it happens the moft often at *Breft* a day after the new or full moon, as the fmalleft tides happen alfo a day after the quadratures. We have obferved at *Dunkirk* and *Havre de Grace*, that the great and fmall tides there, generally happen two days after thefe phafes of the moon, fo that it feems that the preffure which is made upon rhe fea in the new and full moons, and in the quadratures is communicated fooner at *Breft* than at *Havre de Grace* and *Dunkirk*, which appears conformable to reafon; the weftern extremity of *Bretagne*, where *Breft* is fituated, being much more advanced toward the ocean where the preffure is made, than the ports of *Dunkirk* and *Havre*, which are both in the *Englifh* channel.

We

We have remarked already that the various heights which we obferve in the tides, follow pretty exactly the various diftances of the moon from the earth ; that when the moon at the time of new or full, is near its *perigeum*, the tide is greater than in the next *fyzygia*, where it is near its *apogeum*, this is confirmed by thefe laft obfervations; for the 4th of *June* 1712, the day of the new moon, this planet was near its *apogeum*, its diftance from the earth being 1064 parts, the mean of which is 1000. Befides they obferved that day, which was the day of the greateft tide, the height of the full fea to be 16 feet 2 inches, above a fixed point, which they had taken for the bound of the meafures, and that of low-water 2 feet 0 inches, which gives the quantity of the elevation of the tide 14 feet 2 inches. The 19th of *June* the day after the full moon, this planet was near its *perigeum*, its diftance from the earth being 935 parts, the mean of which is 1000. They alfo obferved the 21ft of *June*, the day of the greateft tide, the height of the full fea to be 18 feet 4 inches, above the fixed point; and the height of the low-water 10 inches below this point, which gives the elevation of the tide 19 feet, 2 inches, greater by 5 feet, than in the preceding obfervation, when the moon was near its *apogeum*.

Tho' all thefe obfervations agree in proving that the various diftances of the moon from the earth contribute very much to the various elevations that are obferved in the tides ; we do not pretend that they are the fole caufe of all the variations that are there obferved ; and it even appears, that there are other caufes which may concur in making the height of the tides increafe or diminifh. We do not here fpeak of thofe accidental caufes, which it would be difficult to
give

give rules to; as for example, of the force and fituation of the winds, of the different direction of the fea-coaft, which not only may accelerate or retard the time of the tides, but may alfo caufe different elevations to it. But we only undertake to determine thofe which have fome regular period.

Now in examining all the obfervations which have been made from the 6th of *Feb.* 1712, to the 12th of *July* of the fame year, we find that the higheft tide happened the 24th of *March* at night, and the 25th in the morning when the fea rofe to the height of 19 feet, 1 inch. The lowwater was obferved the 24th in the morning, 1 foot, 6 inches, below the fixed point, fo that the elevation of the fea was, the 24th of *March*, 20 feet, 7 inches. The moon was then nearer its *perigeum* than its *apogeum*, its diftance from the earth being 977 parts, the mean of which is 1000; but we cannot attribute the whole of this elevation of the tide to the proximity of the moon with regard to the earth, fince the height of the tide was obferved the 24th of *March*, greater by 1 foot, 5 inches, than *June* 21, the time when the moon was much neareft its *perigeum*; it appears therefore that there was in the month of *March*, fome other caufe which contributed to the elevation of the tide; and as this obfervation was made near the vernal equinox, which happened the 20th, at 11ʰ 19′ P M. and that of *June* 21, near the fummer folftice which happened at 11ʰ 17′ in the morning: this has given us room to conjecture that all things being equal, the tides are greater in the equinoxes than in the folftices.

In the next new moon which happened on the 6th of *April*, the moon was nearer its *apogeum*

than

than its *perigeum*; its diftance from the earth being 1032, of which the mean is 1000; and *April* the 7th in the morning, they obferved the full fea to be 18 feet, 2 inches; and that of the low-water to be 5 inches, which gives the elevation of the fea for that day 17 feet, 7 inches, fmaller by three feet than the 24th of *March*. And it ought to be fo for two reafons; one of which is, that the moon was farther diftant from the earth the 7th of *April*, than the 24th of *March*, and the other that it was farther diftant from the equinoctial.

Thefe obfervations are confirmed by thofe which were made at *Breft* the preceding year. For the 30th of *June*, 1711, the diftance of the moon from the earth being 960, the elevation of the tide was obferved the 1ft of *July* to be 18 feet, 1 inch, lefs by 2 feet, 10 inches, than the 14th of *Sept.* near the equinox, when it was obferved to be 20 feet, 11 inches, the diftance of the moon from the earth being the 12th of *Sept.* the day of the new moon 969, that is, very little different from that of the 30th of *June*.

It appears therefore by thefe obfervations, that the different heights that are obferved in the tides, depend upon two caufes, whereof the principal, which hitherto is moft confirmed by our obfervations, is the various diftance of the moon from the earth; the fecond is its proximity or diftance from the equinoctial; and that the combination of thefe two caufes produces the chief *phænomena* which we obferve in the height of the tides.

It follows from hence, 1ft, That when the new or full moon meets in its *perigeum*, and at the fame time in the equinoxes, then the tide which immediately follows, is the higheft that is poffible.

2d. That when the new and full moon meet

in the equinoxes, towards the mean diftances, then he height of the tides is greater than in the new or full moons, which happen toward the mean diftances, and near the *folftices*.

3d. That when the new or full moon meets in its*apogeum*, and at the fame time in the *folftices*, then the high-water is the loweft that is poffible. The two firft rules agree with that which we have hereabove marked, and the laft is confirmed by the obfervation of the 4th of *June* 1712, for the moon being then near its *apogeum*, and the fummer *folftice*, the height of the full fea was obferved to be 16 feet, 2 inches, and that of the low-water 2 feet, 0 inches, which gives the elevation of the tide 14 feet 2 inches, which is the leaft that has been obferved at the new and full moons in above a year. The 5th of *July* following the fun was at equal diftance from the *folftice*, but the diftance of the moon from the earth was 1061 a little lefs than the 4th of *June*, which ought to have caufed a greater elevation in the tide, as in effect was obferved. For the 5th of *July* in the morning, the height of the full fea was obferved 16 feet 3 inches, and that of the low-water, 1 foot 8 inches, which gives the elevation of the tide 14 feet 7 inches, greater by 5 inches than the 4th of *June*.

As to the fmall tides which follow the quadratures, they obferve the fame that we have done in the preceding memoirs, that their various elevations depend partly upon the various diftance of the moon from the earth. For example, the 14th of *February* 1712, the day of the firft quarter, the moon being near its *apogeum*, and its diftance from the earth 1062, the height of the full fea was obferved the 15th of *February* at night, 10 feet 9 inches 6', and the height of the low-water 5 feet 2 inches, fo that the elevation of the tide

that

that day was only 5 feet 7 inches 6'. The twenty-ninth of *February* following, the day of the third quarter, the moon being near its *perigeum*, and its diftance from the earth 975, they obferved, the 2d of *March* in the morning, the height of the full fea, 11 feet 9 inches, greater by 11 inches 6 lines than the 15th of *February*.

The 15th of *March*, the day of the firft quarter, the moon being near its *apogeum*, and its diftance from the earth 1063, the height of the full fea was obferved the 16th of *March* in the morning 10 feet 10 inches, and the height of the low-water 6 feet 4 inches ; fo that the elevation of the fea that day, was only 4 feet 6 inches, a little lefs than the 15th of *February*, which ought to happen, the moon being then a little nearer the earth, than in the preceding obfervation of the month.

In the quadratures which happened when the moon was almoft at equal diftance from the earth, they obferved almoft the fame height in the elevation of the tides. For the 12th of *June*, the day of the firft quarter, the diftance of the moon from the earth being 1020, they obferved that 12 in the morning, which was the day of the fmalleft tide, the height of the full fea 12 feet 9 inches, and that of the low-water 3 feet 5 inches, fo that the elevation of the fea, was that day 9 feet, 4 inches.

The 25th of *June* following, the day of the laft quarter, the moon's diftance from the earth being 1028, they obferved the 28th in the morning, the height of the full fea, 12 feet 5 inches 8', and that of the low-water, 3 feet 11 inches 4' ; fo that the elevation of the fea was that day 8 feet 6 inches 4', a little lefs than that of the 28th of *June*, as it ought to have been obferved, the diftance of the moon from the earth, being lefs the 13th of *June*, than the 28th. Thefe two obfervations

having

having been made near the fummer *folſtice*, we have compared them, with thofe which were made near the equinoxes, and found that all things being equal, the fmall tides which follow the quadratures, are greater near the *folſtices*, than near the equinoxes; for the height of the full fea, was obferved the 16th of *March*, the day of the fmalleft tide 10 feet 10 inches, the moon being near its *apogeum*, and the 31ſt of *March*, 13 feet 2 inches, the moon being nearer its *perigeum*. Taking a mean, we fhall have the middle height of the tides, in the equinoxes at *Breſt*, 12 feet, fmaller by 7 or 8 inches, than the mean height drawn from the obfervations of the 15th and 28th of *June*, made in the *folſtices*.

Altho' this effect feems contrary to what we obferved in the new and full moons, the tides of which are greater towards the equinoxes than towards the folſtices; we fee however that it may depend upon the fame caufe; for the moon being in one of its quarters at the time of the equinox, runs over by its daily motion a parallel circle, a little diſtant from the tropicks, and the preffure that it caufes upon the fea, being made by a fmall circle, muſt be lefs felt. On the contrary the moon being in one of its quarters at the time of the folſtice, runs over by its daily motion, the equinoctial, or a parallel very little diſtant from it, and confequently, its preffure upon the waters of the fea, which is made according to a great circle of the earth, muſt be greater than when it run over one of the tropicks.

It appears by this that the feveral heights which are obferved in the tides of the equinoxes and folſtices, muſt not be regulated exactly by the time of the equinoxes and folſtices, but by the greater or lefs declination of the moon, with regard to the

the equinoctial ; for the fun being in the vernal equinox, and the moon in its laft quarter, that is in the fouthern figns, the height of the tide muft be much lefs, when the latitude of the moon is fouthern than when it is northern ; and for the fame reafon, the fun being in the fummer folftice at the time of the new moon, the height of the tide muft be much fmaller when the latitude of the moon is northern, than when it is fouthern.

From thefe obfervations we may draw thefe two general rules.

1ft, That all things being equal, the tides ought to be lefs, when the moon, being in its *apogeum* and in its fouthern figns, its latitude is at the fame time fouthern ; or elfe when the moon being in its *apogeum*, and in the northern figns, its latitude is alfo northern.

2d, That on the contrary, the tides muft be greater, when the moon being in its *perigeum* runs over the equinoctial, without any declination.

It is difficult to find obfervations that have been made exactly, in thefe different circumftances ; it will be fufficient for us to obferve that the 16th of *June* 1711, the day of the new moon, the diftance of the moon from the earth being 1048, that is the moon being near its *apogeum*, with a fouthern declination of 26° 36′, which is the greateft that has been found in more than a year, the height of the full fea was obferved the 17th, to be 16 feet 2 inches 6′, which is one of the fmalleft that has been obferved. The 4th of *June*, the year following, the day of the new moon, the diftance of the moon from the earth being 1064, and the northern declination of the moon 25° 10, the height of the greateft tide was obferved that day, 16 feet 2 inches, which is the leaft that we have

found

found at *Brest*, in the obfervations of the new and full moons.

The fame effect is obferved in the quadratures, for the 5th of *September* 1711, the day of the laft quarter, the moon's diftance from the earth being 1061, and the northern declination of the moon 26° 31', which is one of the greateft that has happened from the 23d of *June* 1711, to the 11th of *July* 1712. The height of the fmalleft tide was obferved the 6th of *September*, 10 feet, 3 inches, which is the loweft that has been obferved for above a year ; the height of the low-water was obferved at night, to be 5 feet 11 inches, fo that the elevation of the fea that day was only 4 feet 4 inches.

The 15th of *March*, the following year, 1712, the day of the laft quarter, the diftance of the moon from the earth being 1063, pretty near the fame as the 5th of *September*, and the moon's northern declination 25° 45', a little lefs than in the obfervation of the preceding year, the height of the fmalleft tide was obferved the 16th of *March*, 10 feet 10 inches, a little greater than the 5th of *September* 1711, but one of the loweft that had been obferved, the elevation of the fea that day being only 4 feet 6 inches.

As to the great tides which ought to happen when the moon is in its *perigeum*, and runs over the equinoctial, we have no obfervations at *Breft* that have been made in thefe circumftances; it will be fufficient to remark, that the 12th of *September* 1711, the diftance of the moon from the earth being 969, and its declination with regard to the equinoctial 2° 39', which is the fmalleft that happened in the new and full moons, from the 16th of *June* 1711, to the 11th of *July* 1712, the height of the full fea was obferved the 14th of *September*,

18 feet 11 inches, and that of low-water 2 feet below the fixed point, which gives the elevation of the tide for that day 20 feet 11 inches, which is one of the greateft that happened at *Brest*, in that interval of time.

We may here obferve that the difference between the circles of declination from one degree to another, continually increafes by receding from the equator, and approaching to the pole ; and that confequently the variation of the declination of the moon, muft produce a lefs fenfible effect upon the tides near the equinoctial than towards the tropicks.

The obfervations here above related, feem fufficiently to prove that the different declinations of the moon, with regard to the equinoctial, contribute to increafe or diminifh the heights of the tides, as well as the various diftances of the moon from the earth. Thefe two caufes being generally complicated, it is neceffary in order to diftinguifh them, to have a greater number of obfervations, made in different fituations of the moon, as well with regard to the earth, as with regard to the equinoctial ; for this reafon we have thought it neceffary to examine fome which were made at *Brest* in 1692.

The journal of thefe obfervations begins *June* 6, 1692, and ends the laft of *October* of the fame year. They have there marked every day the height of the full fea, with regard to a rock called the *Rose*, which is at the entrance and within the port, and have alfo added the temperature of the air, and the fituation and force of the wind.

By the examination that we have made of all thefe obfervations, we have found, that in this interval of time the greateft tide happened the

12th

12th of *September* 1692, two days after the new moon, the sea being that day risen to the height of 28 feet 7 inches. Having calculated the situation of the moon for the time of the preceding new moon, which happened the 10th of *September*, at 6$^h$ 12' PM. we found that this planet was very near its *perigeum*, its distance from the earth being 936 parts, the mean of which is 1000; the moon was also very near the equinoctial, its southern declination being only 3° 11', which could cause very little variation in the height of the sea.

In the other observations of the tides, made at the new or full moons; this planet was not only farther distant from the earth, than on the 10th of *September*, but even its declination, with regard to the equinoctial, was greater ; thus all the tides ought to be smaller than the 12th of *September*, which is conformable to the observations.

In the full moon following, which happened the 25th of *September*, at 10$^h$ 54' in the morning, the moon was in its *apogeum*, its distance from the earth was 1065, and its northern declination 4° 59'. The 25th they also observed the height of the sea, to be 25 feet 5 inches, 3 feet 2 inches lower than the 12th of the same month.

The 14 of *June* the same year, the day of the new moon, the distance of the moon from the earth being 993, the mean of which is 1000, and its northern declination being 27° 10', which is the highest that happened, they observed the 16th the height of the sea, 25 feet 7 inches, lower by 1 foot 5 inches, than the mean height of the tides, drawn from observations made in the month of *September*, when the moon was near the equator.

As to the small tides observed at *Brest*, in the quadratures, we find that the greatest happened

pened

pened the 23d of *June*, two days after the firſt quarter; the ſea being riſen that day to the height of 21 feet 4 inches. The moon was then near its *perigeum*, its diſtance from the earth being 977, it was alſo very near the equinoctial, its northern declination being only 3° 42'.

The 8th of the preceding *June*, the height of the ſea was obſerved 19 feet 1 inch, leſs by 2 feet 3 inches than the 23d of *June*, as they ought to have obſerved, the moon being the 6th of *June*, the day of the laſt quarter, in its *apogeum*, and its diſtance from the earth 1064.

We ſee therefore by theſe obſervations made at *Breſt* in 1692, as well as by thoſe that have been made the laſt years in the ſame port, that the diſtance of the moon from the earth, and its declination, with regard to the equator, contribute very much to the increaſe and diminution that are obſerved in the heights of the tides.

Upon theſe principles we have prepared tables, to find at *Breſt*, the height of the tides, as well in the new and full moons, as in the quadratures.

For this, we ſuppoſe that at *Breſt*, in the great tides which follow the new or full moons, when the planet is in its *perigeum*, and at the ſame time runs over the equinoctial, the height of the full ſea is 20 feet; and that when the moon is in its *apogeum*, and upon the equinoctial, the height of the full ſea is 17 feet 0 inches; and that when it is in its *apogeum*, and its declination with regard to the equinoctial is 28° 50', which is the greateſt that it can have, the height of the full ſea is 15 feet, 6 inches. We alſo ſuppoſe that in the ſmall tides which follow the quadratures, when the moon is

in its *perigeum*, and runs over the equinoctial, the height of the full sea is 14° 0′; that when this planet is in its *apogeum*, and upon the equinoctial, the height of the full sea is 12 feet; and that when it is in its *apogeum*, and its declination is 28° 50′, the height of the full sea is 10° 6′.

According to these rules, the difference of the height of the full sea caused by various distances of the moon from the earth in the new and full moons, is three feet, twice as great as that which is produced by the declination of the moon with regard to the equinoctial.

But as the difference between the distance of the moon from the earth in its *apogeum*, and in its *perigeum*, is smaller by ⅓ in the quadratures than in the new and full moons, they have supposed that the difference of the height of the full sea caused by various distances of the moon from the earth in the quadratures, is only two feet, conformable to the observations.

In the other situations of the moon with regard to the earth; the height of the full sea is proportionable to the various distances of the moon from the earth. To find the variations caused by the declination of the moon with regard to the equinoctial, we have taken declinations of which the sines of the complements are in arithmetical proportion, that we may be able to distribute equally the height of the tides.

We shall find by the means of these tables, and of the following rules, the height of the great and small tides at *Brest*, as well in the new and full moons as in the quadratures.

*Rule*

## Rule I.

To find at *Breft* the height of the greateft tide, which ought to happen in a given new or full moon.

Seek by the aftronomical tables the diftance of the moon from the earth, and its declination with regard to the equinoctial ; take at the top of the firft table the diftance of the moon from the earth, and at the fide its declination, and you will find over-againft it the height of the greateft tide.

## Rule II.

To find at *Breft* the height of the fmalleft tide, which ought to follow one of the quadratures.

Seek by the aftronomical tables, the diftance of the moon from the earth, and its declination ; take at the top of the fecond table the diftance of the moon from the earth, and at its fide its declination, the height of the tide which anfwers to thefe two figures, will be the height of the fmalleft tide which follows the quadrature given.

*Examp.* 1. We feek the height of the greateft tide which happened in the new moon *October* 1711.

We fhall find, that the 12th of *October* at 6° 0′ in the morning, the time of the new moon, the diftance of this planet from the earth was 947, and its fouthern declination 12° 13′. Take in the firft table over-againft 95, and 12° 0′, the height of the greateft tide which

we

we fhall find to be 19° 6' exactly the fame a
was obferved.

*Examp.* 2. We feek the height of the greateft
tide which happened in the new moon of
*June* 1712.

We find, that the 4th of *June* at 7ʰ 25' in the
morning, the time of the new moon, the north-
ern declination of that planet was 25° 10', and
its diftance from the earth 1064. Take in the
firft table over-againft 106, and 25° 33', the
height of the greateft tide that we fhall find 15
feet, 10 inches, only 4 inches lefs than what was
obferved.

*Examp.* 3. We feek the height of the fmalleft
tide which follows the firft quarter of the moon
the 23d of *June*, 1712.

We find, that the 25th of *June* at 8ʰ 18' P. M.
the northern declination of the moon was 6° 30',
and its diftance from the earth 987. Take in the
fecond table over-againft 99, and 6° 56, the
height of the fmalleft tide which we fhall find to
be 13 feet, 8 inches, within about 4 lines of what
was obferved.

*Examp.* 4. We feek the height of the fmalleft
tide which followed the firft quarter of the moon,
*Sept.* 5, 1712.

We fhall find at that time the moon's diftance
from the earth was 1061, and its northern decli-
nation 26° 28'. Take in the fecond table over-
againft 106, and 26° 22', the height of the
fmalleft tide which we fhall find 10° 9' 0", only
6 inches greater than what they obferved.

We

We might draw up like rules to find the height of the tides in the other ports of the ocean, provided we had feveral obfervations of the full fea made in different fituations of the moon, and principally when it is near its *apogeum* and its *perigeum*, when it runs over the equinoctial; or when it is towards its greateft declination.

*A*

A TABLE of the height of the great tides in the new and full moons.

| Declination of the moon. | Distance of the moon from the earth. | | | | | | | | | | | | | |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| | 94 | 95 | 96 | 97 | 98 | 99 | 100 | 101 | 102 | 103 | 104 | 105 | 105 | 105 |
| | Height of the full sea at Brest in the new and full moons. | | | | | | | | | | | | | |
| 0  0 | 20  0 | 19  9 | 19  6 | 19  3 | 19  0 | 18  9 | 18  6 | 18  3 | 18  0 | 17  9 | 17  6 | 17  3 | 17  0 | 16  9 |
| 6  56 | 19 11 | 19  8 | 19  5 | 19  2 | 18 11 | 18  8 | 18  5 | 18  2 | 17 11 | 17  8 | 17  5 | 17  2 | 16 11 | 16  8 |
| 9  48 | 19 10 | 19  7 | 19  4 | 19  1 | 18 10 | 18  7 | 18  4 | 18  1 | 17 10 | 17  7 | 17  4 | 17  1 | 16 10 | 16  7 |
| 12  0 | 19  9 | 19  6 | 19  3 | 19  0 | 18  9 | 18  6 | 18  3 | 18  0 | 17  9 | 17  6 | 17  3 | 17  0 | 16  9 | 16  6 |
| 13 50 | 19  8 | 19  5 | 19  2 | 18 11 | 18  8 | 18  5 | 18  2 | 17 11 | 17  8 | 17  5 | 17  2 | 16 11 | 16  8 | 16  5 |
| 15 26 | 19  7 | 19  4 | 19  1 | 18 10 | 18  7 | 18  4 | 18  1 | 17 10 | 17  7 | 17  4 | 17  1 | 16 10 | 16  7 | 16  4 |
| 16 54 | 19  6 | 19  3 | 19  0 | 18  9 | 18  6 | 18  3 | 18  0 | 17  9 | 17  6 | 17  3 | 17  0 | 16  9 | 16  6 | 16  3 |
| 18 14 | 19  5 | 19  2 | 18 11 | 18  8 | 18  5 | 18  2 | 17 11 | 17  8 | 17  5 | 17  2 | 16 11 | 16  8 | 16  5 | 16  2 |
| 19 28 | 19  4 | 19  1 | 18 10 | 18  7 | 18  4 | 18  1 | 17 10 | 17  7 | 17  4 | 17  1 | 16 10 | 16  7 | 16  4 | 16  1 |
| 20 37 | 19  3 | 19  0 | 18  9 | 18  6 | 18  3 | 18  0 | 17  9 | 17  6 | 17  3 | 17  0 | 16  9 | 16  6 | 16  3 | 16  0 |
| 21 42 | 19  2 | 18 11 | 18  8 | 18  5 | 18  2 | 17 11 | 17  8 | 17  5 | 17  2 | 16 11 | 16  8 | 16  5 | 16  2 | 15 11 |
| 22 44 | 19  1 | 18 10 | 18  7 | 18  4 | 18  1 | 17 10 | 17  7 | 17  4 | 17  1 | 16 10 | 16  7 | 16  4 | 16  1 | 15 10 |
| 23 43 | 19  0 | 18  9 | 18  6 | 18  3 | 18  0 | 17  9 | 17  6 | 17  3 | 17  0 | 16  9 | 16  6 | 16  3 | 16  0 | 15  9 |
| 24 39 | 18 11 | 18  8 | 18  5 | 18  2 | 17 11 | 17  8 | 17  5 | 17  2 | 16 11 | 16  8 | 16  5 | 16  2 | 15 11 | 15  8 |
| 25 33 | 18 10 | 18  7 | 18  4 | 18  1 | 17 10 | 17  7 | 17  4 | 17  1 | 16 10 | 16  7 | 16  4 | 16  1 | 15 10 | 15  7 |
| 26 25 | 18  9 | 18  6 | 18  3 | 18  0 | 17  9 | 17  6 | 17  3 | 17  0 | 16  9 | 16  6 | 16  3 | 16  0 | 15  9 | 15  6 |
| 27 15 | 18  8 | 18  5 | 18  2 | 17 11 | 17  8 | 17  5 | 17  2 | 16 11 | 16  8 | 16  5 | 16  2 | 15 11 | 15  8 | 15  5 |
| 28  3 | 18  7 | 18  4 | 18  1 | 17 10 | 17  7 | 17  4 | 17  1 | 16 10 | 16  7 | 16  4 | 16  1 | 15 10 | 15  7 | 15  4 |
| 28 30 | 18  6 | 18  3 | 18  0 | 17  9 | 17  6 | 17  3 | 17  0 | 16  9 | 16  6 | 16  3 | 16  0 | 15  9 | 15  6 | 15  3 |

## TABLE *of the height of the small tides in the quadratures.*

Distance of the moon from the earth.

Height of the full sea at Brest in the quadratures.

| Declination of the moon. | 98 | 99 | 100 | 101 | 102 | 103 | 104 | 105 | 106 |
|---|---|---|---|---|---|---|---|---|---|
| 0  0 | 14  0 | 13  9 | 13  6 | 13  3 | 13  0 | 12  9 | 12  6 | 12  3 | 12  0 |
| 6 56 | 13 11 | 13  8 | 13  5 | 13  2 | 12 11 | 12  8 | 12  5 | 12  2 | 11 11 |
| 9 48 | 13 10 | 13  7 | 13  4 | 13  1 | 12 10 | 12  7 | 12  4 | 12  1 | 11 10 |
| 12  0 | 13  9 | 13  6 | 13  3 | 13  0 | 12  9 | 12  6 | 12  3 | 12  0 | 11  9 |
| 13 50 | 13  8 | 13  5 | 13  2 | 12 11 | 12  8 | 12  5 | 12  2 | 11 11 | 11  8 |
| 15 26 | 13  7 | 13  4 | 13  1 | 12 10 | 12  7 | 12  4 | 12  1 | 11 10 | 11  7 |
| 16 54 | 13  6 | 13  3 | 13  0 | 12  9 | 12  6 | 12  3 | 12  0 | 11  9 | 11  6 |
| 18 14 | 13  5 | 13  2 | 12 11 | 12  8 | 12  5 | 12  2 | 11 11 | 11  8 | 11  5 |
| 19 28 | 13  4 | 13  1 | 12 10 | 12  7 | 12  4 | 12  1 | 11 10 | 11  7 | 11  4 |
| 20 37 | 13  3 | 13  0 | 12  9 | 12  6 | 12  3 | 12  0 | 11  9 | 11  6 | 11  3 |
| 21 42 | 13  2 | 12 11 | 12  8 | 12  5 | 12  2 | 11 11 | 11  8 | 11  5 | 11  2 |
| 22 44 | 13  1 | 12 10 | 12  7 | 12  4 | 12  1 | 11 10 | 11  7 | 11  4 | 11  1 |
| 23 43 | 13  0 | 12  9 | 12  6 | 12  3 | 12  0 | 11  9 | 11  6 | 11  3 | 11  0 |
| 24 39 | 12 11 | 12  8 | 12  5 | 12  2 | 11 11 | 11  8 | 11  5 | 11  2 | 10 11 |
| 25 33 | 12 10 | 12  7 | 12  4 | 12  1 | 11 10 | 11  7 | 11  4 | 11  1 | 10 10 |
| 26 25 | 12  9 | 12  6 | 12  3 | 12  0 | 11  9 | 11  6 | 11  3 | 11  0 | 10  9 |
| 27 15 | 12  8 | 12  5 | 12  2 | 11 11 | 11  8 | 11  5 | 11  2 | 10 11 | 10  8 |
| 28  3 | 12  7 | 12  4 | 12  1 | 11 10 | 11  7 | 11  4 | 11  1 | 10 10 | 10  7 |
| 28 50 | 12  6 | 12  3 | 12  0 | 11  9 | 11  6 | 11  3 | 11  0 | 10  9 | 10  6 |

## III. *On the height of the atmosphere, by M.* de la Hire*.

Let the circle TNI†, be one of the great circles of the earth, of which the centre is C, and let the meeting of the plane of this circle, with the surface of the atmosphere, which I suppose spherical, be the circle ZPA, of which the centre is also in C; let the sensible horizon of a point T, of the surface of the earth be TL, which shall be perpendicular to the vertical line ZTC of the same point T.

We know by the observations that a ray of light which comes from a star, and makes with the horizon TL, an angle of 32′, underneath, as PM, after having traversed the atmosphere, comes to the eye at T, where it appears in the horizon TL; and this ray must describe in the atmosphere a curve, which shall be touched at the point T, by the horizon LT; for the ray MP, meeting the surface of the atmosphere obliquely in P, draws nearer the perpendicular CP, by advancing toward T, and turning its concavity toward the earth, because the atmosphere is a body which continually increases in density as it approaches the earth. This angle of 32′ is what we call the horizontal refraction.

It follows from hence, that all the luminous points which should be upon the curve PT, within the atmosphere would appear in the horizon TL, and those which should be underneath would not appear from the point T, and if we would consider the atmosphere, as a matter quite homo-

* Feb. 25, 1713.  † Plate VI. Fig. 1. Page

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geneous

geneous like water, and as many aftronomers have done, it is evident the fame thing would always follow ; for then a ray *m p* which fhould make with TL an angle *m p* L 'of 32' by meeting the atmofphere in *p* would turn afide by the right horizontal line p T ; but this fuppofition not being natural, we fhall here make ufe of the curve PT.

If thro' the point P we therefore bring the ray of the earth CNPK which meets its furface in N, and the horizon TL in K, and take an arch N I equal to the arch NT, it is evident that I K would be perpendicular to the femi-diameter of the earth CI ; and that the eye being placed at I would fee all the luminous points which fhould be within the atmofphere, and in a curve I P like TP, in the line IK which would be horizontal from the point I, as TK is from the point T.

But moreover if we alfo imagine beyond I another curve IA within the atmofphere, and which is equal and fimilar to IP or TP, it follows that a ray of light as DA, which fhould meet the atmofphere in A, and make with KI prolonged an angle of 32', would traverfe the atmofphere by the curve AI in touching the circumference of the circle of the earth TNI in I, and would purfue its way by the other curve IP, and would go out of the atmofphere at P, making with IK an angle of 32'.

Now let us fee the effect of the rays of the fun in the atmofphere.

All aftronomers agree that when the centre of the fun is 18 degrees below the horizon, we fee the beginning or the end of the twilight ; and thofe who have obferved the twilight in ferene cold weather, take notice that its light is diftinct enough toward the horizon to make an exact de-

termi-

termination of it; and confequently if we draw the line TF which makes with TL an angle of 18 degrees, this line TF will tend to the centre of the fun without having regard to the refraction ; and if DA was a ray which came from the centre of the fun, and met without refraction TL in Q, the angle DQL would be 18 degrees.

But the beginning of the twilight which may appear to the eye placed at T, is produced by the firft rays which come from the edge of the fun, and may meet the furface of the atmofphere in P upon the curve TP; and thefe rays would make with the horizon TL an angle as *d*QL of 18' *minus* the femi-diameter of the fun; for thofe which fhould make an angle greater than *d* QL, could not meet the arch of the atmofphere in the curve TP, and thofe which fhould make an angle lefs than *d* QL, as when the centre of the fun is nearer the horizon than 18 degrees, would meet arch of the atmofphere between Z and P, and then the light of the *crepufculum* would be already raifed upon the horizon. Altho' we here confider the rays of the edge of the fun for the formation of the twilight, this does not hinder us from having determined its beginning when the centre of the fun is yet 18 degrees below the horizon.

But if *d* Q is the firft ray of the fun which can meet the atmofphere in P, to make the eye at T perceive the beginning of the twilight, it muft come out of its upper limb, which is remote from its centre in the mean diftance of 16'; for this reafon the angle of 18° muft be diminifhed by thefe 16'; and as we have juft demonftrated that it muft be alfo diminifhed 32", which is the horizontal refraction ; we muft therefore fubftract from 18° the two angles of 16' and 32", to have

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the angle of 17° 12', which is the only one we here confider, or want for our purpofe.

Let us fuppofe therefore to avoid the confufion of the lines, that the ray *d*A, which meets the atmofphere in A where it enters, being prolonged to TL, at Q makes the angle *d* Q L of 18° *minus* 16', but that in meeting the atmofphere at A, it turns afide and touches the furface of the earth in I, and afterwards purfues its way by the curve line IP fimilar to I A, which is alfo fimilar to TP ; and if thro' the point *I* we draw the tangent RIK to the arch of the earth TI, which meets TL in K, we fhall have the angle IKL of 17° 12', which muft be equal to the angle TCI, or ACK, and CK will divide this angle into two equal parts; therefore the angle TCK, or ICK, will be of 8 degrees 36', which is the half of 17° 12', when the firft rays of the fun will meet the furface of the atmofphere in P, which is the point where we begin to fee the twilight from the point T placed upon the furface of the earth.

\* Now if we fuppofe that the arch of the atmofphere paffes in K, then the ray as n K, which fhould make with LK an angle of 32', muft be refracted in the atmofphere by the right line KT to go to T, which cannot be as we have faid, becaufe of the unequal denfity of the atmofphere ; therefore the atmofphere muft neceffarily pafs below K towards N.

But if thro' the point T we draw a line T *q f* which makes with LT the angle K T *f* of 32', I fay that the atmofphere cannot pafs in *q*, for then the curvity of the ray *f q* muft be made above *f q* T to go to T, where it muft touch KT,

\* Fig. 2,

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becaufe this curvity muft have its concavity turned toward the earth, and this refracted ray $q$ T being bent, would divaricate from the direct ray $fq$ by receding from the perpendicular C $q$ drawn to the furface of the atmofphere, whereas it ought to approach it ; it will pafs therefore above $q$ toward K.

Let us now fee by the means of the given angles, and by the magnitude of the radius or femidiameter of the earth, which is known, what are the magnitudes of the lines NK and N $q$.

We have in the triangle CTK rectangular in T, the angle TCK of 8° 36′, therefore its complement CKT will be 81° 24′; and we know the femi-diameter of the earth CT 3,269,297 toifes, we fhall find therefore CK of 3,306,520 toifes, from which taking away CN or CT, there will remain NK of 37,223 toifes.

Again, in the triangle CT$q$ we have the angle TC$q$ known as above to have 8° 36′; but the angle C$q$T is lefs than a right one by 36′, fince KT$q$ is fuppofed to be 36′, therefore CT$q$ will be 89° 24′; and laftly, the angle CT$q$ will be 82° 0′; and CT is the fame as in the other ; we fhall find therefore C$q$ 3,301,244 toifes, from which taking away the value of CN, there will remain for N$q$ 31,947 toifes.

It therefore follows from hence, that the height of the atmofphere is lefs than 37,223 toifes, and greater than N$q$ which is 31,947 toifes ; and if we take the mean which cannot be far from the truth, we fhall have the height of the atmofphere NP, 34,585 toifes.

M. *Mariotte* in his effay on the nature of the air, concludes the height of the atmofphere a little lefs than this by his principle of the condenfation of the air in its different heights, and upon experiments:

riments : but thefe forts of calculations can never be very juft, becaufe they are deduced from fome gravity of the air near the earth ; and befides we cannot know by our experiments exactly to what height the elaftic particles of the air can dilate themfelves in the *æther*, nor the progreffion of their dilatation ; and it is a good deal to have come fo near as he has done.

*Kepler* in his *Epitomy of Aftronomy* determines the height of the atmofphere by the *crepufcula* according to the idea of the ancients, who confidered only direct rays which met the atmofphere, after having touched the earth, without having regard to the refraction ; and he finds it by a calculation which he makes of 10 *German* miles, each of which is equal to about 3,800 of our toifes ; and the height of the atmofphere would therefore be 38,000 toifes, which is more than we have found it, and much more than he believed it ; for he reckoned its true height only at a little more than half a *German* mile, which would be almoft 2,000 toifes. There is a great probability that fo confiderable a difference made him feek the means of explaining the twilight, by employing thereupon fome reflections within the atmofphere which he adds to the refraction ; and by a matter which he imagines to be about the fun, and to be illuminated by it ; and he infifts ftrongly upon this thought, for he relates many reafons to fupport it, one of the principal of which is, the curved figure of the twilight which is obferved in the cold and ferene nights ; he alfo adds, that this apparent figure is a fegment of a circle which terminates at the horizon ; but I am going to demonftrate that it is not a portion of a circle, but an hyperbola which is a little altered by the refraction, and that it is not neceffary for the explaining this curved

figure

figure of the *crepufculum*, to have recourfe to a matter which encompaffes the fun, and that it is only the atmofphere which muft produce it, for fuch a matter would very much interrupt the ce-leftial *phænomena*.

† Let there be as before, the vertical line CT, which paffing thro' a point T of the furface of the earth, meets the furface of the atmofphere ZX at the point Z, and let TL be the horizon of the point T, which is perpendicular to the vertical CT; and let the furface of the atmo-fphere ZX be illuminated in P by the rays of the limb of the fun which is yet under the horizon; and as we fuppofe this point P raifed above the horizon TL, there will appear a part of the arch of the twilight above the horizon. If we draw therefore the *radius* of the earth CP which meets its furface at N, and take the arch NI of 8° 36', we fhall have the point I, upon the circumference of the earth, where the rays of the upper limb of the fun, after being refracted in the atmofphere by entering into it, and in their way, will touch this circumference, and from thence turn them-felves toward P, where they will meet the ex-tremity of the atmofphere, as has been feen in the beginning of this memoir.

But as the fame thing happens all about the earth; as to the ray IP in the atmofphere that fur-rounds it, if we draw the *radius* of the earth CI, and thro' the point P the line PV parallel to CI; this line PV will reprefent the circle of the fur-face of the atmofphere which is illuminated by the rays of the edge of the fun; it will therefore be this circle PV, which muft reprefent the arch of the extremity of the twilight feen above the hori-

† Fig. 3.

zon from the point T, of which the point P will be the moft elevated part.

Let us now confider the circle PV for the bafe of a cone which has its *vertex* at T, where the eye is placed, the triangle TPV will be the triangle through the axis of this cone, which is perpendicular to its bafe, and its furface determines the extremity of the twilight with relation to the fummit T ; we need therefore only confider the figure of the fection of this conic furface upon a plane.

When one is pretty far diftant from a plane upon which there is a figure drawn, and cannot know the diftance of the eye from the extremities of the figure, we always imagine that this figure is in a plane perpendicular to the principal ray which goes out of the eye to the figure, as if it had a circle drawn upon a plane, and this plane was very much inclined to the eye which fhould look upon the circle ; and befides the plane not being vifible, and this circle being very diftant from the eye, we fhall always judge that the figure is elliptic, for we conceive it in a plane perpendicular to the rays drawn from the eye to the figure.

It is the fame here with the fection of the cone TPV, of which there can only be a fmall part above the horizon TL ; and as we fuppofe that the twilight is in a vertical plane perpendicular to the horizontal line TL, and to the triangle thro' the axis of the cone, we muft imagine that its figure is upon this plane, and is an hyperbola; fince the plane parallel to the plane where the fection is, and which paffes thro' the *vertex* T of the cone is within. The eye being placed at T, muft therefore judge that the figure of the arch of the twilight is hyperbolical. But alfo the curvity of an hyperbolical arch being flatter on both fides its

*ver-*

*vertex* than the curve of an arch of a circle, will appear alfo more flatted toward the horizon, where the refraction will raife this arch much more than towards its *vertex*, which makes the difference ftill greater between the curvity of the twilight, and an arch of a circle.

It is feen by this explication, that in proportion as the upper part of the arch of the twilight rifes toward the zenith Z, which is done in a very little time, fince it muft run over a quadrant of a circle whilft the fun rifes only 8° 36' toward the horizon, as from the point I toward N in *fig.* 1. that is from the beginning of the appearance of the twilight, and then the part of the horizon occupied by the twilight, is greater than a femi-circle, fince it muft be determined by the little circle Zy which will make with CZ an angle equal to CPV, which cuts the horizon in t beyond T, where its light hinders the eye at this point T from difcerning the term of the arch of the twilight, which very readily difappears from the eye by the rays which come to it from the whole body of the fun.

It ftill remains for me to explain how *Kepler* has found the height of the atmofphere by the twilights to be 10 *German* miles according to the ancients, without having regard to the refraction.

† Let there be the furface of the earth IT, its centre C, and the horizon TK, from the point of its furface T, if we take the arch TI of its circumference of 18 degrees, and draw the tangent DI to the horizon TK at K, we fhall have the point K where we ought to begin to fee the twilight from the point T, for the point K muft be

† Fig. 4.

that

that where the rays of the fun meet the atmo-fphere. It follows therefore, that if we draw CK, which muſt cut in two equal parts in N the arch TI, the angle TCI, or ICK will be 9 degrees, and the triangle ICK will be rectangular in I, and fuppoſing the radius of the earth CI to be 904 *German* miles, he finds CK to be 914 miles, from which taking away CN of 904, there remains NK of 10 miles for the height of the at-mofphere.

He adds, that this demonſtration neglects all the caufes except the fun, as the refraction, the reflection of the rays within the atmofphere, and the etherial matter which he imagines about the fun: thus he reafons from thefe caufes without applying any calculation to them, to draw from them the height of the atmofphere of a little above half a mile, which he reckons to be its true value.

He fays, that the matter of the atmofphere is homogeneous, and that its furface is as well terminated as that of the ocean. It is certain, that the refraction of the rays of the fun in entering into the atmofphere, muſt be confidered as I have done; but he adds that thofe rays which do not meet the earth, and which in purfuing their way within the atmofphere, meet its furface in going out as if they had entered it, except fome which are reflected within toward the earth, and which only produce a very faint light, and thefe laſt which are reflected by meeting again the furface of the atmofphere, go out of it alfo; except fome which reflecting themfelves again within the atmofphere, go to meet its furface in a place which may be feen from the earth, which produces in that place the appearance of the twilight. It is eafy to judge that thefe few rays which fhould

touch

touch the atmofphere after feveral reflections, and fhould be almoft all gone out of the atmofphere, as he himfelf faid, could not make impreffion upon the fight, and chiefly meeting only the furface of the air, which is a very rare body, altho' he fuppofes it homogeneous like water.

He afterwards runs out into a great length to prove by fome calculation which he makes, the neceffity of the matter which he has imagined about the fun, which muft produce the curvity of the twilight ; but all this is of no fervice, and is intirely ufelefs for this phænomenon, as I have before demonftrated.

I have already explained why we cannot diftinguifh exactly the bounds of the twilight, when it is pretty much elevated above the horizon ; and even when it is yet toward the horizon, it very often happens alfo that we cannot fee it well terminated ; for if the firft rays of the fun which form it, as d I in the firft figure, pafs through thick clouds, or vapours, which are upon the furface of the earth toward I, they will be turned from one fide to the other, and do not make in P a light ftrong enough to fee there diftinctly the term of the *crepufculum*, and this commonly happens. For towards the morning, where the fun beginning to inlighten fucceffively the furface of the earth, a great many vapours arife ; and towards evening, thofe which had rifen in the day time, fall upon it again by the abfence of the fun which forfakes them ; it will not be the fame thing if the air is very cold, and without clouds, it is alfo that time alone when the twilight is well terminated in its beginning, or in its end. We muft alfo add to this, that toward the beginning of twilight, the eye which is at T, fees the particles of air, which are enlightened very near one another

other

other, which caufes a much livelier appearance of light, than when it is raifed above the horizon, where we only fee thefe enlightened particles very much feparated, which cannot ftrike the eye ftrong enough to diftinguifh the term of the *crepufculum*, and much lefs when the enlightened particles are toward the zenith Z.

In fine, if it is not granted that the beginning or end of the twilight appears when the fun is yet 18° below the horizon, and that it was at 17° or 19°, we muft only increafe or diminifh the height of the atmofphere, as I have juft found it about 2000 toifes for each degree.

Nor do I pretend that the height of the atmofphere which I have fuppofed, muft be the fame over all the earth as toward the equator, or towards the poles; but that depends upon obfervations which might be made in thofe countries; and I am even perfuaded that in the countries towards the poles, the height of the atmofphere is much greater than in thefe, or I believe greater than toward the equator; but towards the poles the obfervations of it may be very well determined, becaufe of the great cold and clearnefs of the air, which reigns there in winter.

Here is an obfervation which will ferve to confirm the height of the atmofphere as I have juft determined it. In 1676 there appeared in fome parts of *Italy*, a meteor, which was as bright as the full moon. M. *Montanari*, profeffor at *Bolonia*, made fome obfervations on it, and having compared them with thofe which had been made in other places, he determined the height of this meteor to be 15 mean leagues of *France*, which he printed in a fmall work, intitled *Fiamma volante.*

We

We cannot doubt but that all thefe fires or me-
teors are formed by fulphureous exhalations which
come'out of the earth,and becoming inflamed,weigh
much lefs than the particles of air of which they
occupy the place ; but let them be ever fo light,
they are neverthelefs heavier than the other which
we confider as without any weight. For this rea-
fon they muft raife themfelves juft upon the fur-
face of the atmofphere, where they float as long
as they laft ; thus the height of thefe fires muft be
the fame with that of the atmofphere, and confe-
quently the 15 leagues of height obferved of this,
which comes to 35,000 toifes, confirms what I
have found for the height of the atmofphere.

## IV. *Of the figure of the earth, by M.* Caffini.

We fhall not here undertake to relate the various
opinions which have divided philofophers con-
cerning the figure of the earth.

We cannot imagine any figure which they have
not attributed to it ; for not to mention thofe who
believed it to be like a column, a drum, a cone,
or a tree, the root of which was infinitely extend-
ed ; fome have judged it to be flat without ad-
mitting any other inequality in it, than what is
caufed by the mountains.

Others being afraid leaft the waters of the fea
fhould run out if they were not reftrained by fome
bounds, gave it the figure of a concave hemi-
fphere.

Laftly, others confidering that the tops of the
towers, and high mountains were perceived at a
diftance, while their bafes were concealed below
the horizon ; that thofe who were in the moft ele-
vated places faw the fun rife earlier, and fet later,
than thofe who were in the loweft places ; that the
fhadow

shadow of the earth appeared in the eclipses of the moon to have a circular figure ; and that those who travelled from north to south, saw the southern stars rise above the horizon, whilst the northern stars were depressed, judged it to be spherical.

This opinion which was founded upon solid reasons, was almost generally received by those who undertook before us to determine the magnitude of the earth by geometrical operations. They made use of the measure of a small portion of its circumference, to conclude by that its whole extent, by supposing that all the degrees of the meridians of the earth were equal among themselves ; and that the lines perpendicular to the horizon, which pass through the zenith, and measure the degrees in the heavens, were directed towards the same point, which they judged to be the centre of the earth.

Several great geometricians of our time have abandoned this hypothesis of the sphericalness of the earth. Sir *Isaac Newton* in his *Philosophiæ Naturalis principia Mathematica*, having considered that the force which he calls centrifugal, and which results from the daily motion of the earth, must raise at the equator those parts which endeavour to recede from the axis of the earth ; judged that it must be depressed toward the poles, and he found according to his principles, that supposing the earth to be of an uniform matter, as dense at its circumference as towards its centre, the diameter of the equator; must be to the diameter which passes through the poles as 692 to 689.

M. *Huygens*, in his discourse of the cause of gravity, having considered that at *Cayenne*, which is only 4 or 5 degrees distant from the equator, a pendulum which swings seconds is shorter there than at *Paris* by a line and $\frac{1}{4}$, from whence it

fol-

follows, that if we take pendulums of equal length, that of *Cayenne* makes the vibrations a little flower than that of *Paris*: judged that the caufe of this phœnomenon could be referred only to the daily motion of the earth, which being greater in each country, according as it approaches nearer the e-quinoctial line, muft produce a proportionable effort to reject the bodies from the centre, and con-fequently take from them a certain part of their gravity. He adds, that this effort which refults from the circular motion of the earth, muft make a plummet, fufpended to a cord, vary from the perpendicularity ; and as the furface of all liquids is fo difpofed, that the line of fufpenfion is per-pendicular to it, becaufe otherwife it might de-fcend more, it follows, that the fea has the figure of a fpheroid, and that the earth muft have con-formed itfelf to it, when it was collected by the effect of gravity. Upon thefe principles he ad-vances as a paradox, that the earth is not perfectly fpherical, but of the figure of a fphere flatted to-wards the poles, nearly fuch as an ellipfis would make by turning about its leffer axis ; and he concludes, that the diameter of the equator ex-ceeds the axis of the earth by $\frac{1}{578}$, whereas accord-ing to Sir *Ifaac Newton*, this excefs is but $\frac{1}{230}$ of the diameter of the equator.

But M. *Einfenfchmid*, a celebrated mathemati-cian of *Strasburg*, having examined the magni-tude of the degree, which refulted from feveral dimenfions, made under different parallels by va-rious mathematicians, found that the magnitude of the degree of the earth, drawn from the mea-fures of *Snellius* made in *Holland*, was fmaller than that which M. *Picard* had determined by his obfervations made in *France*, which is more fouth-ward; that the magnitude of the degree, which

<div align="right">refults</div>

refults from the meafures of M. *Picard*, made in the neighbourhood of *Paris*, was fmaller than that which F. *Riccioli* found at *Bolonia*, which is more fouthern than *Paris* ; and that this was yet fmaller than the magnitude of the degree, which had been formerly determined by *Eratoftenes*, between the city of *Alexandria* and that of *Syene*, which was under the tropic of *Cancer*; this in quailty of degrees, which increafed in magnitude, as they approached the equinoɗial line, made him judge that the earth was not fpherical, but had the figure of a fpheroid prolonged toward the poles, the meridians of which are reprefented by ellipfes, and the equator and parallels by circles.

He determines upon this foundation the inequality of the degrees of the meridian of the circumference of the earth ; but he owns that it were to be wifhed, that fome of thefe obfervations were again made near the pole, and the equator; to determine more exaɗly and with more certainty the figure and fize of the earth ; and that the meridian line, which ought to be drawn through the royal obfervatory on each fide, to the confines of the kingdom, would be of very great importance for the deciding this queftion.

In his *Treatife of the figure of the earth*, he relates alfo the opinions of feveral authors, who, as well as he, have judged that the earth was prolonged towards the pole ; and he recites among others, Dr. *Burnet*, who confidering that by the daily revolution of the earth, the mafs of water receives a greater degree of velocity toward the equator, than towards the poles, where it defcribes fmaller circles ; the parts of water which are moſt agitated, make an effort to remove from the centre of their motion, and cannot raife themfelves, becaufe of the air which furrounds them on

all

all fides and refifts them ; fo that they are obliged to flow on both fides to put themfelves *in equilibrio*; for, adds he, the waters which find any obftacle, flow on the fide where they find a paffage, and where their motion is moft free, fo that by the diminution of the waters of the fea, which are toward the equator, the globe of water is a little prolonged towards the poles, and the cruft of earth which is formed upon it, muft take the fame figure.

In this diverfity of opinions concerning the figure of the earth, we have thought it neceffary to examine what that is which refults from the obfervations which we have made in the fouthern part of *France*, compared with thofe that M. *Picard* had made in more northern places.

M. *Picard's* meafure of the earth extends from the parallel of *Amiens*, which is 49° 54' 46" to the parallel of *Malvoifine*, which is 48° 31' 48", and in this fpace, which is about 1 degree and $\frac{1}{3}$, the magnitude of the degree of a meridian amounts to 57060 toifes.

Our meafures begin at the royal obfervatory of *Paris*, which is under the parallel of 48050' 10", and end at *Collioure*, which is toward the fouthern extremity of *France*, under the parallel of 42° 31' 13". In this extent, which is 6° 18' 57', we have found the magnitude of each degree one with another 57100 toifes. Thus if we fuppofe M. *Picard's* obfervations and ours to be exact in all their circumftances,the refult is that the degrees, which are toward the north, are lefs than thofe which are toward the fouth; and that confequently the figure of a meridian of the earth, muft be fuch that the degrees fhould encreafe the more they approach the equator, and diminifh on the contrary in going toward the poles ; which is the property

1                                                                          of

of an ellipfis, the great diameter of which repre-
fents the axis of the earth, and the leffer diameter
that of the equator, as will be demonftrated here-
after.

This ellipfis turning about its great axis, forms
by its revolution a fpheroid, of which the poles
are at the extremities of the great axis, and the e-
quator and parallels are reprefented by circles.
This figure is what we attribute to the earth, and
we fhall give according to this hypothefis, a very
fimple method to divide the ellipfes, which repre-
fent the meridians of the earth divided into degrees
and minutes, and determine the inequality of thefe
degrees, which are terminated in the heavens by
perpendiculars to the horizon, which pafs through
the zenith, and cut all of them the axis of the earth
in different points. Let * BDCR be an ellipfis
which reprefents a meridian of the earth, of which
let the poles B and C be at the extremity of the great
axis BC, and let the foci E and F be taken at dif-
cretion.

We would divide this ellipfis into degrees, that
is, find feveral points H, I, V, fuch, that the di-
ftance from the pole to the zenith of each of thefe
points be a certain number of given degrees, fuch
as we pleafe.

Let there be drawn from one of the foci of the
ellipfis E, the line ET, which makes, with the
axis BC, an angle BET, equal to the diftance
given from the pole to the zenith. Let there be
taken with a pair of compaffes, a fpace equal to
the axis BC; and from the other focus F, as a
centre, let there be defcribed at this interval, an
arch of a circle, which cuts in T the line ET; I

* Plate VI. Fig. 5.

fay, that the line FT, drawn from the point T to the focus F, will cut the ellipfis at the point H, which is fuch, that the diftance from the pole to the zenith of this place, will be of the number of degrees given.

## Demonstration.

From the point H let there be raifed HZ, perpendicular to the ellipfis, which paffes through the zenith z, and being prolonged on the infide, meets the axis of the earth in O, and divides by the property of the ellipfis, the angle EHF in two equal parts. Let there be alfo drawn from the point H, HP parallel to the axis BC, and directed to the pole P, which we fuppofe at an infinite diftance. The angle PHZ or POZ, meafures the diftance from the pole to the zenith of an inhabitant, who fhould be upon the earth at the point H.

By the conftruction, F T is equal to the axis BC; but BC by the property of the ellipfis, is equal to EH, *plus* HF; taking away the common FH, we fhall have EH equal to HT. The angles ETH, TEH, will therefore be equal, and confequently each half of the external angle EHF; but the angle EHO is alfo equal to half the angle EHF; the angles TEH, EHO, will therefore be equal between themfelves, and confequently the lines ET, HO, will be parallel between themfelves, and the angle POZ, which meafures the diftance from the pole to the zenith, from the point H, will be equal to the angle BET, which, by the conftruction, was made equal to the diftance given from the pole to the zenith. Q. E. D.

Now

Now if we suppofe the proportion of the grea diameter of the ellipfis BC, to the diftance EF between the *foci*, fuch as we pleafe, we may determine by calculation all the points of the ellipfis as H, which terminate the degrees, making as FT or BC is to EF ; fo is the *fine* of the angle PET, the given diftance from the pole to the zenith, to the fine of the angle ETF or TEH, the value of which will confequently be known. This angle TEH being added to the angle PET, the given diftance from the pole to the zenith, from the point H, gives the value of the angle BEH ; that the line drawn from the focus to the fought point H, makes with the axis of the ellipfis.

Now in the triangle EHF, of which the fide EF is known, as well as the angle EHF, which is double of the angle TEH, and the angle FEH fupplement of the angle BEH ; we fhall have the value of the fide EH known in parts of the axis BC.

By the fame method we fhall find the angles BEI, BEV, *&c.* and the value of the lines EI, EV, for the diftance from the pole to the zenith, of all the degrees of the circumference of the earth ; and in the rectilineal triangles HEI, IEV, of which the fides HE, EI, EV are known as well as the angles contained between the fides HEI, IEV, which are the difference between the angles BEH, BEI, BEV determined here above, we fhall know the value of the chords HI, IV, contained between each degree.

We fhall therefore have the exact proportion of the chords of each degree of the circumference of the earth in the elliptical hypothefis. And as the proportion of thefe cords between themfelves is not fenfibly different from the propor-

tion

tion between the arches of the ellipfes which they fubtend, we fhall have at the fame time the proportion between the degrees of the circumference of the earth at any diftance from the pole, fuppofing the excentricity of the earth to be in a certain quantity.

For a greater exactnefs we might calculate the proportion between the chords of the halves and quarters of a degree of the circumference of the earth, that the difference which might be between the proportion of the arches and that of the chords, was lefs fenfible ; but if we confider that the excefs of the arch of a degree upon the chord which fubtends it, is only about 4 feet ; it is eafy to judge, that the difference between the proportion of the chords of each degree, and that of the arches of the ellipfes which they fubtend, is abfolutely infenfible ; add to this, that the meafures which we have ufed for determining the magnitude of the earth, have been made according to right lines, and not according to the curvity of the circumference of the earth.

Having applied the method juft explained, to the figure of the earth, which we at firft fuppofed to be like the orb of the moon in the elliptical hypothefis, and of which the diftance between the *foci*, is, according to the modern aftronomers, about $\frac{1}{23}$ of the great diameter ; we have found that according to this hypothefis, the degrees increafe in magnitude, in receding from the pole and approaching to the equator, agreeable to our obfervations, but that this augmentation from one degree to another, at the diftance from the pole of 40 degrees, was but two toifes and 4 feet, which is too little to reprefent the inequality of the degrees,

degrees, which refults from the comparifon of our obfervations with that of M. *Picard's*.

We have therefore been obliged to fuppofe the excentricity of the earth greater, and we have found, that by eftablifhing the diftance between the foci of the ellipfis, which reprefents a meridian of the earth, double to that which is attributed to the orb of the moon ; fo that this diftance may be to the great diameter of the earth, as 8,724 to 100,000, that is pretty near as 1 to 11. This ellipfis perfectly reprefents the figure of a meridian of the earth, fuch as refults from our obfervations compared to thofe of M. *Picard*.

We find for example, that the degree contained between the parallels of 49 and 50 degrees, being 57,060 toifes, according as it has been determined in the meafure of the earth ; that which is between the parallels of 50 and 51 degrees, is 57,071 toifes, and 2 feet, greater than the preceding by 11 toifes, 2 feet, and that the fum of fix degrees contained between the parallels of the places which we have determined the diftance of, is 342,600 toifes, which gives the magnitude of thefe degrees, one with another, 57,100 toifes, as we have obferved.

We alfo find, that according to the hypothefis of the elliptic earth, the fmalleft inequality from one degree to another, is toward the poles, and the equator where it is only two or three feet. This inequality in the degrees increafes afterwards on both fides to the parallel of 45 degrees where it is the greateft that is poffible, and which we have calculated to be about 11 ½ toifes.

It follows from hence, that the moft convenient places to know if there be any inequality in

the

the degrees of a meridian of the earth, are contained between the parallels of 40 and 50 degrees, which are exactly thofe which we have determined by our obfervations. This inequality in the degrees muft be obferved toward the parallel of 45 degrees, even tho' we fhould fuppofe the earth to be depreffed toward the poles; with the difference that the degrees diminifh in magnitude in approaching the equator, which is contrary to our obfervations.

By continuing this inquiry, we find the magnitude of a degree of a meridian toward the pole to be 56,785 toifes and $\frac{1}{2}$, and that of a degree near the equator 57,440 toifes, fo that from the greateft to the leaft degree of the earth, there is 655 toifes difference.

Taking the fum of all the degrees of a meridian, we fhall have its circumference of 20,560,295 toifes, only 4,295 toifes greater than the circumference of the earth would be fuppofing it fpherical, when the magnitude of the degree is 57,100 toifes.

With regard to the axis of the earth, we fhall find it 6,557,040 toifes greater than in the fpherical hypothefis by 13,856 toifes, or about feven of our leagues. We fhall have alfo the diftance between the two *foci* of the earth 286,018 toifes, or 443 leagues; and in the rectangular triangle DAE, of which the fide AE, the half of the interval between the two *foci*, is known as well as the hypothenufe ED, which by the property of ellipfes, is equal to half the great diameter BC, we fhall find AD to be 3,266,020 toifes, of which the double DR, the diameter of the equator will be 6,532,040 lefs than the axis BC, by 25,000 toifes, or 12 or 13 of our leagues.

x                                                    The

The difference between the axis of the earth, and the diameter of the equator, will therefore be the $\frac{1}{262}$ of this diameter, greater by half than what M. *Huygens* has determined, and pretty near the same with Sir *Isaac Newton*'s, but contrary ways.

The diameter of the equator being known, we shall have its circumference of 20,521,006 toises, which gives the magnitude of the degrees of the equator which in this hypothesis are equal between themselves, of 57,003 toises almost the same with that of the meridian which is 36 degrees distance from the pole.

Taking the difference between the circumference of a meridian which we have found to be 20,560,295 toises, and that of the equator which is 20,521,006 toises, we shall have 39,289 toises, or about 20 leagues, by which the circuit of the earth about one of these meridians, exceeds its circuit about the equinoctial.

According to these principles we might determine in toises, or leagues, the diameter and circumference of each parallel; for in the rectangular triangle ELH, the angle LEH, and the hypothenuse EH, being known, we shall find the value of the side LH semi-diameter of the parallel which passes thro' the point H, the latitude of which is known.

The magnitude of the meridians and parallels of the earth being thus determined with regard to our observations, they may be used in the construction of terrestrial globes, and geographical maps.

To facilitate the description thereof, we have drawn up a table, in which is marked in toises,

and feet, the magnitude of all the degrees of the meridians from the poles to the equator.

This table will ferve to compare not only the obfervations which have already been made at different diftances from the pole to determine the magnitude of the earth; but alfo thofe which may hereafter be made with the fame defign under feveral other parallels.

*A*

## A TABLE of the degrees of the meridians of the earth.

| Height of the pole. | Dist. from the pole to the zenith | Degrees of a meridian. | | Height of the pole. | Dist. from the pole to the zenith | Degrees of a meridian. | | Height of the pole. | Dist. from the pole to the zenith | Degrees of a meridian. |
|---|---|---|---|---|---|---|---|---|---|---|
| D. | D. | Toises. | Feet. | | | Toises. | Feet. | | | Toises. | Feet. |
| 90 | 0 | 56785 | 3 | 60 | 30 | 56952 | 5 | 30 | 60 | 57280 | 1 |
| 89 | 1 | 56785 | 5 | 59 | 31 | 56962 | 5 | 29 | 61 | 57289 | 5 |
| 88 | 2 | 56786 | 4 | 58 | 32 | 56973 | 1 | 28 | 62 | 57299 | 2 |
| 87 | 3 | 56787 | 5 | 57 | 33 | 56983 | 3 | 27 | 63 | 57308 | 4 |
| 86 | 4 | 56789 | 3 | 56 | 34 | 56994 | 0 | 26 | 64 | 57317 | 5 |
| 85 | 5 | 56791 | 3 | 55 | 35 | 57004 | 5 | 25 | 65 | 57326 | 3 |
| 84 | 6 | 56793 | 5 | 54 | 36 | 57015 | 4 | 24 | 66 | 57335 | 1 |
| 83 | 7 | 56796 | 3 | 53 | 37 | 57026 | 3 | 23 | 67 | 57343 | 2 |
| 82 | 8 | 56799 | 4 | 52 | 38 | 57037 | 4 | 22 | 68 | 57351 | 2 |
| 81 | 9 | 56803 | 1 | 51 | 39 | 57048 | 5 | 21 | 69 | 57359 | 0 |
| 80 | 10 | 56807 | 0 | 50 | 40 | 57060 | 0 | 20 | 70 | 57366 | 2 |
| 79 | 11 | 56811 | 2 | 49 | 41 | 57071 | 2 | 19 | 71 | 57373 | 2 |
| 78 | 12 | 56815 | 5 | 48 | 42 | 57082 | 4 | 18 | 72 | 57380 | 1 |
| 77 | 13 | 56820 | 5 | 47 | 43 | 57094 | 0 | 17 | 73 | 57386 | 4 |
| 76 | 14 | 56826 | 1 | 46 | 44 | 57105 | 3 | 16 | 74 | 57392 | 5 |
| 75 | 15 | 56831 | 4 | 45 | 45 | 57117 | 0 | 15 | 75 | 57398 | 3 |
| 74 | 16 | 56837 | 5 | 44 | 46 | 57128 | 2 | 14 | 76 | 57404 | 0 |
| 73 | 17 | 56844 | 1 | 43 | 47 | 57139 | 4 | 13 | 77 | 57409 | 0 |
| 72 | 18 | 56850 | 5 | 42 | 48 | 57151 | 1 | 12 | 78 | 57413 | 4 |
| 71 | 19 | 56857 | 5 | 41 | 49 | 57162 | 2 | 11 | 79 | 57418 | 0 |
| 70 | 20 | 56865 | 0 | 40 | 50 | 57173 | 4 | 10 | 80 | 57422 | 0 |
| 69 | 21 | 56872 | 4 | 39 | 51 | 57184 | 5 | 9 | 81 | 57425 | 3 |
| 68 | 22 | 56880 | 3 | 38 | 52 | 57195 | 5 | 8 | 82 | 57428 | 4 |
| 67 | 23 | 56888 | 4 | 37 | 53 | 57206 | 5 | 7 | 83 | 57431 | 3 |
| 66 | 24 | 56897 | 1 | 36 | 54 | 57217 | 5 | 6 | 84 | 57433 | 5 |
| 65 | 25 | 56905 | 5 | 35 | 55 | 57228 | 3 | 5 | 85 | 57435 | 5 |
| 64 | 26 | 56914 | 5 | 34 | 56 | 57239 | 1 | 4 | 86 | 57437 | 3 |
| 63 | 27 | 56924 | 0 | 33 | 57 | 57249 | 4 | 3 | 87 | 57438 | 4 |
| 62 | 28 | 56933 | 4 | 32 | 58 | 57260 | 0 | 2 | 88 | 57439 | 3 |
| 61 | 29 | 56943 | 0 | 31 | 59 | 57270 | 1 | 1 | 89 | 57440 | 0 |
| 60 | 30 | | | 30 | 60 | | | 0 | 90 | | |

V. *Experiments and reflections upon the pro-*
*digious ductility of several bodies, by M.*
de Reaumur; *translated by Mr.* Cham-
bers.

Art has its wonders as well as nature, and the
reason why we do not perceive them is, that
they are always before our eyes. It is enough
for us to supply our wants, or our luxury, with-
out enquiring into the artifice we are beholden to
for it.———Of this we have an instance in the
wier-drawer's art: the person that wears lace or
embroidery, seldom considers the formation of
those wonderful threads, but a few philosophers
have examined them, and have drawn excellent
proofs from them of the prodigious divisibility of
matter which they might have carried still further,
if they had been acquainted with the full effect
of the wier-drawer's art. They have even at-
tempted to account for this extreme ductility of
metals, though one of the greatest secrets in na-
ture, as depending on that very obscure property
hardness, and having its obscure difficulties be-
sides.

As to the philosophy of ductility, we have
little to offer more than has already been done,
but as to the management and application of it by
art, we are in a condition to discover several
things, accordingly, having had occasion to make
several experiments on this head; we shall here
produce them, and examine the effects of ducti-
lity in very different bodies. It will afford no dif-
agreeable spectacle to see collected, as into one
point of view, all that is remarkable on a subject
wherein

wherein art and nature feem to ftrive which fhall furnifh moft curiofities.

Ductile bodies are thofe, which when beat, preffed, or drawn, ftretch without breaking one way, almoft what they loofe in another: fuch are metals, which, by hammering, gain in length and breadth, what they loofe in thicknefs, or by being drawn thro' a wier-drawer's iron, become longer in proportion, as their thicknefs is lefs; we have alfo another kind of bodies, which tho' not malleable like metals, may yet be accounted ductile, as glue, gums, refins, and all bodies which being foftened by water, fire, or any other diffolvent, will draw out into threads.

———— Hence ductile bodies may be divided into 2 claffes, the firft whereof comprehends thofe which are hard and malleable, of which we fhall treat in the firft place; the fecond confifts of the foft bodies, which tho' not malleable, are capable of being drawn out.

The common method of extending the firft kind is, by beating them with a hammer: thus it is the artificers in gold, filver, copper, and pewter, reduce maffes of thofe metals into what figure they pleafe; but tho' this kind of manufacture deferves more attention than is commonly beftowed on it, we fhall not dwell thereon, our defign being only to confider ductile bodies with regard to the great extenfion they are capable off.

The gold beaters, by bare means of a hammer, reduce that metal into plates inconceivably thin: thus it is they prepare thofe leaves commonly ufed in guilding, which all come from a thick ingot, gradually made thinner and thinner, till at length the leaves become light enough to be blown away by the flendereft puff: to learn in fome furer way, than by the accounts of the

work-

workmen, to which *Rohault* confided, how
far gold may actually be stretched in this method,
I took a quantity of the thinneft leaves, fuch as
thofe in their common books, and after meafuring
them carefully, weighed them in a fine ballance,
by which means I found, that a grain of gold was
here beaten into an extent of 36 fquare inches $\frac{1}{2}$,
and 24 fquare lines, on which footing, an ounce
of gold, which, in a cubic form would only have
meafured 5 lines $\frac{1}{3}$, either in breadth, length, or
depth, and which would only cover a furface of
about 27 fquare lines, when ftretched by the
gold-beater, will cover upwards of 146 fquare
feet $\frac{1}{2}$, a degree of extenfion greater by almoft $\frac{1}{2}$,
than they were able to give leaf-gold 90 years
ago. It was looked on with furprize in father
*Merfenne*'s time, that an ounce of gold might be
made into 1600 leaves, which altogether, only
covered 105 fquare feet.————It would be te-
dious here to explain how the art arrived at its
prefent perfection. We fhall not fo much as
mention the ingenuity of the operators, who,
from bullocks gutts have procured that delicate
parchment without which gold could not be re-
duced into leaves fo thin.

After all, how confiderable foever the exten-
fion of gold may be in the leaf, it will appear
but a fmall matter, when compared with that
which the gold wier-drawers give it. There are
fome leaves of beaten gold, which in certain pla-
ces are not $\frac{1}{30000}$ of a line thick ; but $\frac{1}{30000}$ of a
line is a confiderable thicknefs, when compared
with the gold which covers the filver wier, com-
monly fpun upon filk.

To conceive the extent to which gold is then
ftretched, and how prodigioufly thin it muft be,
it will be neceffary to have a general idea of the

gold-

gold-beater's art.——What we call gold wier, e-very body knows is only filver wier gilt, and is drawn from a thick bar of filver; they take fuch bar, weighing about 30*lb.* and rounding it into the form of a cylinder or roller, about 15 lines in diameter, and 22 inches long; they gild it with leaves prepared by the gold-beaters, the leaves however employed for this purpofe are thicker than thofe intended for common gilding, and they frequently apply feveral, one over another, but tho' the layer of gold be here thicker than in other gildings, yet is it very thin, as may eafily be inferred from the quantity of gold therein. To gild thefe 30 *lb.* of filver, they never ufe above fix ounces of gold, which is enough even to make a fuper-gilding; but the common proportion is only 2 ounces; and they frequently fcarce allow one, where the wier is to be but flightly gilt, as in the common gold wier of *Lyons.* Upon the whole, the layer of gold, which covers the ingot, is ne-ver above $\frac{1}{15}$ of a line thick, frequently only a 30th or 45th, and fometimes barely a 90th part.

Yet how much thinner muft this thin layer of gold be ftill brought, and how many times divi-ded and fubdivided! The ingot itfelf cover'd with it, muft be drawn to a great degree of finenefs, e-qual, or even fuperior to that of a hair; this is done by paffing it fucceffively thro' feveral holes, each finer than the other, and in proportion as its diameter is reduced by the flendernefs of the hole, its length is increafed, and confequently its fur-face augmented, yet the gold, which covers the filver ingot, ftill continues to cover it, how prodi-gioufly foever extended, nor ever leaves the fmal-left point bare. Now how many divifions muft it have undergone, when the ingot is reduced into

a

a thread, whofe diameter is 9000 times fmaller than that of the ingot.

To give a further idea of the prodigious ductility of gold, I weighed ½ a drachm of the fineft wier, and meafuring it with care, found it 202 feet, confequently the ounce of wier was equal to 3232 feet, and the pound, or 12 ounces, to 38784; the ingot therefore, which weighed 30*lb.* and was only 22 inches long, had now arrived at the length of 1163520 feet; fo that reducing the feet into fathoms, and taking the league of 2000 fathoms, the 22 inches length will be found extended into 96 leagues and 196 fathoms, an extenfion vaftly fuperior to what father *Merfenne* and *Furetier* mention; the latter of thofe gentlemen obferves after the former, that ½ an ounce of wier will reach 100 fathoms and more; this more is very confiderable, fince 100 fathoms, for ½ an ounce, would only be 1200 feet for an ounce, whereas we have found it 3232 feet. *Rohault* alfo comes far behind us in the extenfion of gold.

But the ingot, tho' reduced into fo flender a wier, is not left here, but remains to be lengthened ftill further; the greater part of the gold wier is fpun upon filk, and before fpinning muft be flatted, by paffing it between 2 wheels of polifhed fteel; and thefe wheels in thus flatting it, draw it out $\frac{1}{7}$ more, fo that the ingot is now arrived at 111 leagues long; thefe leaves of flatted wire are very flender, their breadth is only about $\frac{1}{8}$ of a line, from whence it follows, that their thicknefs is only $\frac{1}{256}$ of a line, as appears by an eafy calculus. For the weight of a cubic foot of gold, and a cubic foot of filver, being known by accurate experiments; we fuppofe here that the cubic foot of gold weighs 21220 ounces, and the cubic foot of

<div align="right">filver</div>

silver 11523, we shall not stand to shew the method whereby it appears that these silver laminæ are only $\frac{1}{256}$ of a line thick : but instead thereof turn our eyes upon the extream thinness of the leaf gold which covers them ; this will appear truly amazing, if we consider the small quantity of gold applied on the silver ingot ; suppose 2 ounces, as we have shewn that less is often us'd by calculation, it will appear, that the surface cover'd by these two ounces of gold, is 2380 square feet, or that an ounce of gold covers 1190 square feet, whereas the gold beaters can only extend it to 146 square feet.

Now how prodigiously thin must the gold thus extended be, by the preceding calculus, we shall find its thickness only $\frac{1}{175000}$ of a line, and yet to have the gold which covers the silver, this $\frac{1}{175000}$, the gold must be supposed equally thick in every part, which yet is contrary to all experience, for how carefully soever the leaf gold be beat, 'tis impossible not to leave inequalities in it, 'tis evident from their greater and less transparency, that they are at least twice as thick in some places than in others, and consequently in gilding the ingot, they will cover it very unequally, leaving the gold thicker by half in certain places than in others ; now if we enquire, the thickness of the gold in those parts where it is thinnest, it will be only found equal to the 262,500th part of a line.

Yet, is not this the utmost bound to which the ductility of gold may be carried ; in lieu of 2 ounces, 1 need only have been used, in which case, the gold which covers the silver wire, would only have been in some places the 525,000th part of a line ; but what is more, this silver-wire for all its thinness might be made thinner by $\frac{1}{2}$,

I

yet ftill remain gilt. All required hereto, is to fqueeze it clofer between the wheels, till the propofed extenfion be effected, and the wire meafure $\frac{1}{4}$th of a line broad; here therefore the gold which covers them will be reduced to the 1,000,000th part of a line thick, in which cafe, their thicknefs makes a lefs part of a line, than a line does of 1,200 fathoms.

It may perhaps be furmized, that the gold is much thicker than appears by this calculus, fince it may be divided into little feparate grains, yet placed fo near each other, as to give its colour to the filver. In fine, it is very natural to fuppofe, that the gold which covered the flatted wire does not form a continued leaf; but experience fhews the contrary; for upon diffolving fome of the gilt threads in aqua fortis, how fmall or thin foever they may be when the menftruum has had its effect, they will be found turned into fo many fmall hollow tubes, the aqua fortis haying eat out the filver, but left the gold intire; whence it appears, that the gold which covered the filver, forms one continued body; and confequently we are able by art to divide a piece of gold, a line thick, into a million different leaves.

The art of extending foft bodies has not yet been carried near fo far. The only body of this kind that we can ftretch any thing confiderably, is glafs; and it will appear extreamly furprifing that a fubftance, which of all others, is the moft brittle, and ftubborn, fhould be laid down as the firft among foft ductile ones; 'tis known indeed, that when fufed by the fire, the workman can fafhion it like foft wax; but what is moft remarkable, and nearlieft concerns our prefent fubject is, that it may be drawn out into threads inconceivably flender, a common fpinner does not form her

<div align="right">threads</div>

threads of flax or hemp with half so much facility, as a glafs-man will make threads of this brittle matter.

Few people but have feen a fort of plumes ufed in the toppings of childrens caps, and in other ornaments, which 'tis well known, are no other than bunches of glafs threads, tho' finer than any hairs, and bending like them, by the fmalleft breath of wind. A manufacture fo very extraordinary, would doubtlefs bear a very high price, were it not fo very fimple, and eafy to be performed ; it employs 2 perfons at the fame time, but fcarce requires any fkill or dexterity in either of them.

The 1ft holds one end of a piece of glafs over the flame of a lamp, and when it is fufficiently foftened, the other applying one end of a glafs hook thereto, draws it away again without feparating it from the reft of the mafs, then fitting this hook upon the circumference of a vertical wheel, about 2 feet $\frac{1}{2}$ in diameter, not unlike a common fpinning wheel, nothing remains but to turn it about ; for in proportion as it turns, it draws part of the melted glafs towards it, which part ftill adhering to thofe before and after them, form a thread which winds round the rim of the wheel ; each revolution lays on a new round of thread, till at length the circumference of the wheel is covered as it were with a skain of glafs thread ; the melted mafs diminifhing apace, and winds out like a clue on the rim of a wheel ; the parts as they recede from the lamp, grow cool, and ftick fafter to each other ; the reft which are nearer the fire remaining loofe : and thus continually obeying the attraction of the former. Nor is the workman obliged to turn his wheel flowly, for fear of breaking the thread ; he may turn as

fwift, and difpatch his work as faft as he pleafes, without the thread breaking at all the oftener.

The threads thus formed, are not uniformly round, but their circumference is a very flat oval, being twice or thrice broader than thick ; fome of them are extremely fine, fcarce fhort of the thread of a filk worm ; and accordingly thefe are flexible to a furprifing degree. One may knot the 2 ends of fuch a thread, and draw them till the hole in the middle be fcarce ¼th of a line in diameter e'er it will break.

How ftiff foever, and inflexible glafs appears in the lump, it is not effentially fo : if we had the art of drawing much finer threads, they would be more flexible in proportion ; whence it may be concluded, that if we could draw threads as flender as thofe wherein the fpiders lay their eggs, we might make glafs threads proper to be wove in ftuffs ; and that if glafs be not malleable, it may however ( if I may ufe the term ) be texable. I have tried feveral ways for making glafs threads vaftly finer than is commonly done, but have not been able to get them to the neceffary length. The danger is in giving too great a degree of fufion to a matter fo very thin, as that which muft here be ufed, not to mention the difficulty of drawing it gently enough, and in an equable manner. The method which fucceeded the beft is, that which follows ; I took a glafs thread feven or eight inches long, and fufpending it in the air by one end, I loaded the other end with a piece of wax hardly weighing a tenth part of a grain, which yet was enough to draw the glafs thread downwards. Near the thread thus fufpended, I drew a little taper ; upon the arrival whereof, at a certain degree of nearnefs, I could perceive the little weight defcend in jerks :

by

by this means I frequently ftretched a piece of thread ¼th of an inch long, to the extent of nine or ten inches, but have rarely been able to go much further, by reafon the flighteft breath of wind would blow the flame of the taper fo near the thread, as to melt and break it; it was not eafy to make a fufficient number of threads in this manner, whereof to compofe one pretty thick one; but by the experiment I learned, that one may make threads of glafs finer than thofe of any filk-worm. Thofe I drew in this manner feemed almoft as fine as a fpider's threads. I would willingly have learned to what degree they were flexible, for they appeared to be prodigioufly fo; but they were too fine, and fhort, and their number too fmall for this purpofe.

Thus much is certain, that the matter whereof, even the fpiders and filk-worms fpin their threads, is brittle in the mafs, like all dry gums, as I have tried, by letting it dry; and it may be added, that tho' the threads fpun hereof were lefs flexible than they are, they might neverthelefs be woven, whence it feems to follow, that we only want the art of extending the glafs, to be able to work it into cloth.———In effect, if the glafs threads were fine enough to give them the flexibility neceffary for weaving, they would naturally be ftrong enough to prove their ftrength. I hung feveral weights to the fineft glafs threads, which the workmen make, and found that one fingle thread would fuftain 15 drachms, or near 2 ounces without breaking; 'tis true thefe threads were 3 or 4 times broader than a thread of a filk-worm, but they did not feem any thicker, whence it feems to follow, that they would be confiderably ftronger than the threads of a filk-worm, even though

they

they were equally flender, fince one of the ftrong-
eft threads of filk will only fuftain 2 drachms ⅓,
whofe ftrength of confequence is only ⅙ of that of
a glafs thread. A difproportion much greater than
that between their folid contents. Accordingly pick-
ing a large bunch of the fineft of thefe threads,
and dividing them into feveral portions, and
twifting thefe together as barbers plait treffes of
hair, we find them very ftrong, not but feveral
other threads will break in the plaiting ; and
after all, there is no likelyhood that any great ad-
vantage will ever be procur'd from glafs threads.

Gums, refins and wax, are alfo ductile foft
bodies, but none of them are much confidered by
artificers in that capacity ; 'tis true the wax-
chandler's draw their torches or tapers through
holes, like thofe of a wire-drawer, but this is not
to extend, but to make them round and fmooth.

If we be defective in the management of the
clafs of ductile foft bodies, 'tis in fome meafure
compenfated by that artifice found in feveral ani-
mals, for extending it beyond all imagination ;
fo that we have nothing to do but employ the
threads prepared for us. 'Tis obvious we here fpeak
of the filk-worm's threads which are formed of a
vifcous matter iffuing from the body of the in-
fect, which arrives at a confiftence much as glafs
threads become hard, by receding from the lamp,
tho' from a different caufe, as fhall be fhewn
hereafter, nor fhall our enquiry into the extenfion
of ductile foft bodies, terminate in the filk of
worms. Thefe animals which yield the moft profit,
rarely afford us the moft curiofity, but nature on
the contrary, feems to have beftowed moft art in
the contrivance of thofe which are troublefome
to us, and for which we even have an averfion ;
thus fpiders, thofe mifchievous infects, which pro-

3                                                    bably

bably would not fpin at all, unlefs to incommode us, as we have already fhewn, in an enquiry into the produce of their filk, in the memoirs for the year 1710, are incomparably better fitted than filk-worms, to fhew how far nature can go in extending a vifcid juice.———In the difcourfe already cited, we only fpoke occafionally of the extream finenefs of their threads, to prevent digreffing too far from the principal point, we fhall now examine a little nearer, and infpect that admirable mechanifm whereby they are formed.

The illuftrious *Malpighi* in his anatomy of the filk-worm, defcribes the parts from whence their filk is derived, and we fhall find another *apparatus* in the body of fpiders———near the fpiders tail there are fix teats or *papillæ* *, the end of each whereof is a fpinning inftrument, through which the threads are drawn ; thefe moulds or fpinning inftruments are inconceivably fmall, in a compafs lefs than a pin's head. There are enough of them to give rife to a furprifing multitude of threads, as may be eafily perceived by their effects ; taking a large garden fpider ready to lay its eggs, and applying a finger on one of its papillæ, as we withdraw the finger, a furprifing quantity of feparate threads will come after it, as fhewn by KAMN, &c. I have endeavoured to learn their number by ufing a good microfcope, and have frequently told upwards of 70 or 80, but could eafily perceive that they were infinitely more, tho' they all arofe from a fmall part of the papillæ. In effect, if I were to fay that the end of each *papilla* furnifhes a thoufand threads, the number would perhaps be thought exceffive, and it appears to me much too fmall to exprefs the number of thefe threads, as every

* Plate VI. Fig. 9.

body

body may be convinced, by examining a *papilla* of a chamber-fpider with a microfcope. In this contemptible infect we find a beautiful ftructure ; the end of the *papilla* is divided into a multitude of fmaller eminences, much like flies or butterflies eyes, and each eminence doubtlefs ferves to make a different thread, or the feveral dents between the eminences may perhaps be pierced, and thus give paffage to fo many threads, the little eminences only ferving to keep the threads from mixing together ; thefe eminences are not fo fenfible as the papillæ of garden fpiders, but in lieu thereof we find a foreft of little hairs, which may probably anfwer the fame purpofes——be this as it will, it appears incontestable, that from each papilla of a fpider, there may arife threads from above a thoufand different places, and confequently the animal having 6 *papillæ*, may give paffage to 6000 threads ; nor is the artifice of nature barely feen in piercing fuch immenfely fmall holes, but the threads are already formed when they arrive at the *papillæ*, each of them having its little canal or vagina apart ; in effect, we find them formed and feparate from each other at a great diftance from the *papillæ*, and to explain the whole mechanifm, we muft go back as far as the fource of the juice they are compofed of.

In infects fo fmall and foft fuch delicate parts are hardly diftinguifhable without a little attention, but boiling the animal, or drying it, or leaving it a few hours in fpirit of wine, renders all the effential parts vifible without a microfcope. Near the rife of the belly, as at DD *, we find two little foft bodies, which are the firft fources of the filk in figure and tranfparency ; they refemble glafs drops, whence for the conveniency of expreffion,

* Fig. 7.

we

we fhall not fcruple to call them drops * ; the point of each drop goes winding as in R, and forms a multitude of bends towards the *papillæ*. From the bafe of the drop proceeds another branch much bigger than the former, as S, which making alfo a greater number of folds, forms feveral knots, and advances like the other towards the hind part of the fpider.

I have fometimes unfolded this laft branch till it were 9 or 10 inches long, tho' only a part of it were then unfolded. The drop and branches which arife from them, contain the matter whereof filk is to be formed ; but this matter is yet foft, and unlefs the fpider have been dried, will hardly draw out into long threads. The body of the drop may be confidered as a refervoir, and the 2 branches as canals proceeding from them. If the fpider be not over-boiled, the branches will be vifibly feen invefted with a membrane which prevents the tranfparency of the liquor from appearing, but by rubbing the canal gently, this membrane comes off a little further ; behind, there are 2 fmaller drops which fend forth one more branch from their point, fo that on each fide the fpider there are 3 drops which fend forth their liquor, by 3 canals, to the proper refervoirs from whence it is immediately fupplied for the formation of filk.

On each fide the fpider, as at EE †, there are three other bodies, which may be confidered as the ultimate refervoirs wherein the liquor is gathered ; thefe we call the grand refervoirs ‖ ; they are much bigger than the drops, and the three which are on the fame fide are ranged in fuch manner by each other, that they only feem

* Fig. 10.　† Fig. 7.　‖ Fig. 11.

to

to form a fingle body, they are very different in figure, and yet have this in common, that each of them makes 6 or 7 bends, and are pretty equably thick throughout, tho' one of their extremities is bigger than the other, the larger end as VVV, is next the head of the infect, and the fmaller TTT, next the *anus*; thefe latter terminate in a point, and almoft meet each other, like the three middle fingers of the hand. From the three points of thefe refervoirs proceed the threads, or at leaft the chief parts of the threads, which iffue at the three *papillæ*, each refervoir being deftined to fupply one *papilla*; this is not difficult to perceive, for not only the point of each of thefe refervoirs is feen terminating in a thread; but if the adjacent parts be dexteroufly managed, we perceive a multitude of threads which proceed from thefe bodies, and may be traced to the *papillæ*.

In fine, at the origin of the *papillæ* we difcover feveral flefhy tubes, and have reafon to imagine, that there are as many *papillæ*. If the membrane or pellicle which feems to cover thefe tubes be removed, we find them full on the infide, with threads all diftinct from each other, and which of confequence, under one common cover, had each of them their particular one.

'Tis true, upon tracing thefe threads we find feveral of them which come further off than the points of the grand refervoirs, fome of them feem to come from the middle, others a little lower, and others a little higher; fo that the whole multitude of threads collected near the papillæ of the fpider do not all feem to arife from the points of the refervoirs; 'tis more probable that fome arife from the feveral angles, or perhaps from other

parts

parts of the reservoirs. All we can affirm for certain is, that these bodies seem to have one common tegument, and that there are a multitude of threads which follow all their windings.

As to the manner wherein this juice is gathered in the drops, and its passage thence into the grand reservoirs, the eye can give us no information. *Malpighi*, as clear sighted as he was, contented himself with describing the vessel wherein the liquor of his silk-worm is collected, without either explaining the manner wherein it comes thither, or its passage out again. What then can be expected from us, in an insect much smaller than the silk-worm, and which contains 6 or 7 thousand times more parts? 'Twill be enough for us to make some reflections on the prodigious ductility of the matter whereof their threads are composed, and the immense finenes of the holes, thro' which they pass, and the tubes they are moulded in.

We have observed, that from the end of each *papilla* there may issue upwards of a thousand threads, yet this end of a papilla is no broader than a pin's head, and the holes are of necessity separated from each other by intervals which must be greater by much than the holes themselves; and this even in the largest spiders: for if we consider the young ones produced by these, we shall find them to spin threads the moment they quit their nest; these threads 'tis true are hardly to be perceived; but the webs formed of them are sufficiently visible, being frequently as thick and as close as that of a chamber-spider; and this by reason 4 or 5 hundred young spiders concur to the same work. How small now must the holes be thro' which these threads are drawn? The imagination

can

can hardly reach the fmallnefs of one of thefe papillæ. The whole fpider perhaps is fcarce fo big as a fingle papilla of one of the parent fpiders, as may be eafily fhewn ; for every pregnant fpider lays 4 or 5 hundred eggs, which are all inclofed in a ball ; and as foon as a young fpider has broke thro' this ball, they begin to fpin. How flender then muft each of the threads be that iffue from each papilla ! It were perhaps needlefs to fhew, that nature can carry the ductility of this matter ftill further, otherwife it might eafily be done ; for fome fpiders are fo fmall at their birth, that there is no diftinguifhing them without a microfcope. They are of a red colour, and being gathered in large crowds, feem only to the naked eye, like clufters of red fpecks ; and yet all of them, how imperceptible foever, make webs, and confequently fpin. What then muft the tenuity be of the threads which iffue from the holes of their papillæ ? A hair compared to one of thefe threads muft be thicker than the largeft bar or ingot compared to the flendereft filver-wire. To fay no more, thefe threads, which however are confiftent, and hang together, are lefs in diameter than the thinneft layer of gold which covers the fineft filver-wire.

We have already obferved, that the matter whereof thefe threads are formed, is a vifcous juice ; its firft appearance is in the drops above-mentioned, where it has but little confiftence ; but we find it much improved in the grand refervoirs, to which it is tranfmitted from the drops. We fuppofe a confiftence required in the way by a fecretion of part of the moifture, or aqueous liquor mixed therewith, made by parts deftined for that purpofe ; but the liquor dries ftill further in

its

its paffage to the papillæ, thro' proper canals, till at length it becomes thread. At its appearance without the hole, it ftill remains foft and glutinous, fo that the threads of different holes ftick together at fome diftance therefrom, and the matter does not become perfectly dry, till the air has exhaled the reft of its moifture.

Thus much is eafily proved by drying a large fpider by the fire, or boiling it in water. A little drying or boiling will give the matter in the drops a fufficient confiftence to fit it to be drawn out into threads, and at the fame time will unfit that in the grand refervoirs from being drawn; but boiling the fpider to a further degree, the matter in the drops becomes a kind of hard glue, incapable of any further drawing; whence it appears evident, that it is by drying or evaporating the ufelefs moifture, that the matter of filk becomes filk.

I was well nigh betrayed indeed by a fpecious experiment into an opinion, that it is not by the evaporation of an aqueous matter, that the threads of filk acquire their confiftence. Drawing out fome threads from the hind part of a fpider, and winding them on a ftick, I plunged the fpider, ftick and all, in water, and continuing to turn the ftick about, found the filk threads continue to iffue from the papillæ as before.————I was not yet in the fecret of the method of the fpider's fpinning, nor knew that the threads had acquired a confiftence before they iffued from the holes: this confiftence, tis true, is yet defective, but fuffices to make them hang together; and they continue to dry in the water. A proof hereof is, that if the drops, or the grand refervoirs, be fteeped in cold water, the contained

matter

matter gains no confiftence thereby ; on the contrary, if the matter, either of the drops, or the grand refervoirs, be left a while in fpirit of wine, it will acquire the fame confiftence, as if the fpider had been dried.

We may add, that the matter in thefe refervoirs refembles filk in its colour, tho' in no other quality. 'Tis much like a gum, or a tranfparent glue, and breaks when bent to a certain degree, or a certain number of times, like glafs. It is only flexible when divided into very flender threads ; and if nature have multiplied the number of holes in the papillæ, 'tis probably becaufe the filky matter formed in their bodies, is naturally more brittle than the matter of filk-worms; and requires to be divided in fmaller parts. Without this, what neceffity was there for forming fuch a number of feparate threads to be united again after, when a fingle canal might have made a thread big enough at once ?

'Tis probable, that the matter in the refervoirs never grows perfectly dry, even in the air ; but that the middle parts ftill remain a little moift ; the external furface muft neceffarily dry the firft ; and this furface, when dried, is indiffoluble by water, and impenetrable by it ; and confequently muft prevent their moifture in the middle of the mafs from efcaping, as well as the external moifture from entering ; and this probably was one of the reafons why fpiders threads are naturally fo fmall, fince to make them ftrong they muft be very dry ; and they never would have been fufficiently dry if they had been lefs flender.

It would now be time to explain from whence the prodigious ductility of this matter arifes, but it is rather time to defift, the length of our me-
<div align="right">moir</div>

moir being but ill attoned for by clofing the recital of certain facts without reafonings, which are always precarious.

*An explanation of the figures in plate* IV.

*Fig*. 6. is one of the fpecies of fpiders, which fpin filk, feen at the upper part.

*Fig*. 7. the fame fpider feen underneath. A A mark the place where the belly is joined to the breaft.

At B are the *papillæ* and the *anus*.

DD fhew the place, where the firft refervoirs, which we call drops, are placed.

EE fhew the place where the laft refervoirs are.

*Fig*. 8. is a part of the belly of a fpider magnified.

F is the *anus*.

GGGG are the 4 great *papillæ*, vifible only when the fpider holds them as in this figure, which it commonly does when one holds it.

*Fig*. 9. is alfo a part of the belly of a fpider magnified ; it fhews all the *papillæ* difpofed as they are when one preffes the belly of the infect.

HH are the two moft diftant *papillæ* from the *anus*. A bundle of threads *a* comes out of one of thefe *papillæ*.

II are two great *papillæ*, almoft equal to the former.

KK are two fmaller *papillæ*.

L is a flefhy part which covers the backfide of the fpider, it raifes it up, to let the excrements go out.

OMP

OMP are 3 *papillæ* magnified ; different fpecies of fpiders have them differently made.

O is one of the *papillæ* H.

M is one of the *papillæ* I. It appears that a prodigious number of feparate threads N proceed from this.

P is one of the middle *papillæ* K ; all the forts have a quantity of hairs ; but it would have made them too confufed, if we had put all that the microfcope difcovers.

*Fig.* 10. RQS is one of the refervoirs which we have called drops ; the part Q is the neareft to AA in *fig.* 7. R is part of the drop ending in a point. S is that which makes a net-work, before it arrives at the great refervoirs.

*Fig.* 11. are 3 of the great refervoirs placed one above another, but not quite fo near as they are naturally. Our view was to fhew that they are three. In the animal they feem at firft fight to make but one body. VVV are the neareft ends to the head of the infect.

TTT are the fineft ends, and the neareft to the *anus*.

*Fig.* 12. is one of thefe refervoirs feen feparately.

## VI. *Reflections on the obfervations of the tides by M. Caffini\*.*

Philofophers are not yet agreed as to the caufe and effects of the flux and reflux of the fea.

*Poffidonius*, according to *Strabo*, fays, that the motion of the fea imitates the revolution of the

\* Aug. 2, 1713.

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heavenly bodies, and that there is in the flux of the fea a daily motion, another which follows the revolution of the lunar months, and an annual one.

That the diurnal motion is that which the fea makes in rifing and falling twice in the day, that the monthly motion fhews itfelf by the different heights of the tides, which are great toward the new moons, diminifh to the firft quarter, and increafe to the full moon, after which they fall again. As for the annual motion, he had learned from the inhabitants of *Cadiz*, that toward the fummer folftices the fluxes and refluxes of the fea were the greateft, which made him conjecture, that they diminifh to the autumnal equinox ; that they afterwards increafe to the winter folftice, after which they diminifh, and fo on fucceffively.

*Pliny* affirms, that the fun and moon are the caufe of the flux and reflux ; he feems to be of the fame opinion with *Poffidonius*, as to the daily motion of the flux and reflux of the fea, and in that which is obferved in each revolution of the moon ; but he affirms on the contrary, that the greateft tides happen in the equinoxes, and the fmalleft in the folftices ; and that they are alfo greater in the autumnal equinox, than in the *vernal*. He adds, that the tides are lefs when the moon is northern, and diftant from the earth, than when it is fouthern, and its force acts nearer ; and that in the fpace of eight years, after 100 revolutions of the moon, the fame principles of the motion of the tides, and the fame augmentations are obferved. He obferves laftly, that all thefe changes do not happen exactly in the times here above marked, but fome days after. The effects of things which

pass in the heavens are not felt upon the earth so soon as they are seen.

Several modern philosophers have also discovered three sorts of motions in the flux and reflux of the sea ; one which it makes twice every day ; another which follows the periods of the moon ; and the third which is seen every year at the time of the equinoxes and solstices.

They all agree with *Pliny* in supposing that the tides are greater in the equinoxes than in the solstices ; but they differ among themselves in what concerns the cause of these *phænomena*.

*Galileo* is of opinion, that the principal cause of the flux and reflux of the sea, proceeds from the motion of the earth about its axis, which it makes in 24 hours, during which it is carried on at the same time by its annual motion, which it makes about the sun in the space of a year. Altho' both these motions are made from the west to the east, each point of the surface of the earth must have different degrees of velocity with relation to a fixed point taken in the heavens. For by the daily revolution of the earth, the parts exposed to the sun are carried off a different way to that by which the earth is moved by its own motion ; and, on the contrary, the parts of the surface of the earth which are in the opposite hemisphere to the sun, are carried off by the daily revolution the same way by which they are carried on by its own motion ; from whence there results a compound motion, the velocity of which is greater than in the preceding case, and varies according to the different directions of these two motions. The parts of the surface of the earth being therefore moved sometimes slower, and sometimes faster, in the space of 24 hours, it follows, that

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the waters contained in the fea, which cannot exactly follow the motion of the furface of the earth, are obliged to flow and ebb in the fpace of a day, juft as water contained in a veffel would do, which being carried away with a certain degree of velocity, on a certain fide, would flow back on the oppofite fide, and afterwards return toward the other edge when this velocity fhould begin to abate confiderably. From hence he concludes, that there muft be in it a flux and reflux in the fpace of 24 hours; but becaufe of the water which always tends to be in *equilibrio*, and feveral accidents which may happen, as the different depths of the fea, and of the direction of the coafts of the fea which interrupts its motion, the flux may accelerate 2, 3, 4, 5, or 6 hours, which makes them commonly obferve in the *Mediterranean* the flux of the fea every fix hours, though in other places we may find it different.

With regard to the motion of the tides which follow the periods of the lunar months, he pretends that they are produced by the inequality of the motion of the earth, which acquires a greater degree of velocity when the moon is in conjunction than when it is in oppofition. For this he fuppofes, that the force proceeding from the fun acts in like manner at an equal diftance from it, and moves with more velocity the bodies which are nearer the centre of their motion, than thofe which are farther diftant; from whence it follows, that the moon muft have a greater degree of velocity in its conjunction, when it is nearer the fun, than in its oppofition, when it is farther diftant from it, which alfo contributes to accelerate or retard the motion of the earth, like a pendulum, which makes quicker or flower vibrations, according as

the plummet is placed nearer or farther from the centre of its motion. This inequality of the motion of the earth in the conjunctions and oppositions, the period of which is the same as that of the revolution of the moon, is the cause, as he imagines, of the inequalities that are observed in the tides in the course of a month.

As for the inequalities which are observed in the tides in the course of the year, *Galileo* imagines that they proceed from the difference which results from the composition of the annual and daily motion, according to the different situations of the earth upon the ecliptic. For the daily revolution being made about the poles of the equator, and the annual revolution about the poles of the ecliptic, which is 2 3 degrees and ½ distant from it, it follows that when the earth is in its tropicks, the circle of the declination concurs with the circle of a latitude which is called colure, and both these revolutions are made in the same direction ; whereas, when the earth is in the equinox, these motions are inclined to one another in the direction of 2 3 degrees and ½, from whence there proceeds a different composition of motion from that which happens when the earth is in the tropics. According to this author, there results from the difference of these two compound motions some inequalities which are the cause of that which is observed in the solstices and equinoxes.

From hence he concludes, that the periods of the tide which are regulated according to the days, months, and years, all have for their first and principle cause the annual and daily motion of the earth ; and that the sun and moon come in only by accident.

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*Defcartes*, who feems better informed than *Galileo* in the *phænomena* of the tides upon the coafts of the ocean, and who knew that they there obferved regularly the flux and reflux of the fea twice a day, in the fpace of 24 hours, 48 minutes, or thereabouts, fo that the fea takes exactly 6 hours 12 minutes in rifing, and as much in falling, attributed the principal caufes of this phœnomenon to the motion of the moon.

He judged that the celeftial matter, which furrounds the earth being moved by the daily motion with more velocity than the earth, was inclofed between the earth and moon, which obliged the earth to give way a little to the oppofite fide. That by this means its waters were compreffed on both fides, according to the direction of the moon to the earth, which made them flow back on both fides to the diftance of 90 degrees, where the greateft height of the fea was. That the moon being arrived 6 hours, 12 minutes afterwards, at the diftance of 90 degrees from the place where it was before the waters, which had been then raifed, were compreffed by the interpofition of the moon, and the fea confequently was lower there than in any other place. And therefore it muft have in the fame place a viciffitude of high and low fea in the fpace of 12 hours, 24 minutes, as has been obferved there.

As to the tides which in each lunation are higheft in the new and full moons, and loweft in the quadratures, he pretends that this proceeds from the direction of the vortex of the earth, which according to his fyftem, is elliptical, and has its leffer axis directed toward the fun. That in the conjunctions and oppofitions the moon is in this direction, and confequently the flux and reflux of

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the fea muft be greater than in the quadratures, when the moon is fituated in the direction of the greateft axis of the ellipfis.

He adds at laft, that the moon being always in a plane near the ecliptic, and the earth making its daily revolution according to the plane of the equator, both thefe planes interfect one another in the equinoxes, whereas in the folftices they are very diftant from one another; from whence it follows, that the greateft tides muft happen toward the beginning of the fpring and autumn.

*Kepler* in his lunar aftronomy * attributes the caufe of the flux and reflux of the fea to the bodies of the fun and moon, which attract the waters of the fea by a virtue almoft like that of the load-ftone. He owns it is difficult to explain by this means why the flux of the fea is as great at midnight when the fun and moon are abfent, as at noon when they are prefent. However, he imagines that the flux of the night may be produced by the reflection made againft the coaft of *America* by the waters, that the moon has carried away with her, and reciprocally by the reflection made againft the coafts of *Europe* and *Africa*, by the water that the moon brought at her return.

Sir *Ifaac Newton*, in his *Principia Mathematica* adopts the opinion of *Kepler*, in attributing the caufe of the tides to the attraction produced by the fun and moon. According to his principles he finds, that the fea muft rife twice every day, both folar and lunar; and that the greateft height of the tide muft happen in lefs than fix hours after the paffage of the fun and moon through the meridian, as is obferved in the eaftern parts of the

* Page 70.

Atlantic

*Atlantic* and *Ethiopic* oceans, between *France* and the *Cape* of *Good-Hope*, and upon the coasts of *Chili* and *Peru* of the *Pacific* ocean where the flux of the sea happens much about the third hour.

These two motions which the sun and the moon produce, are not perceived distinctly, but make a compound motion. In the conjunctions and oppositions these two effects are joined together, and form the greatest flux and reflux. In the quadratures the sun raises the water in the place where the moon depresses it, and the fluxes and refluxes of the sea, which result from this difference, are the least that can happen in the course of a month, and because according to experience the effect of the moon is greater than that of the sun, the greatest height of the sea must happen at the third lunar hour. He calls the lunar hour the 24th part of the time that the moon takes up in returning to the meridian of the same place.

Sir *Isaac Newton* also judges that the effects of the sun and moon are greater in their least distances from the earth than in the greatest, and that in a triple ratio of the apparent diameters. That consequently, all things being equal, the sun being in the winter in its *perigeum* the tides must be a little greater than in summer; and that the moon being in its *perigeum*, the tides must be greater than 15 days before or after, when it is in its *apogeum*.

He adds, that the effect of the sun and moon depends upon its declination or distance from the equator. That if both these planets were in the direction of the pole, they would attract all the waters uniformly; so that there would not be any
reciprocal

reciprocal motion; and therefore the fun and moon in receding from the equator towards the poles, lofe by degrees their effort, and make the tides fmaller in the fyzygies of the folftices, than in thofe of the equinoxes; but in the quadratures of the folftices, the tides muft be greater than in the quadratures of the equinoxes, becaufe the effect of the moon which is then in the equator exceeds that of the fun.

The greateft tides therefore happen in the fyzygies, and the fmall in the quadratures which are about the time of the equinoxes, and the greateft tide of a fyzygy is followed by the leaft of a quadrature, which he fays agrees with experience. It alfo proceeds from the diftance of the fun from the earth, which is lefs in winter than in the fummer, that the greateft and fmalleft tides oftener precede the vernal equinox than follow it; and oftener follow the autumnal equinox than precede it.

Sir *Ifaac Newton* finds afterwards, that the effects of the fun and moon depend alfo upon the latitude of places; that we may confider the fea as divided by the flux of the fea into two hemifpheroides, one of which is toward the north, and the other toward the fouth; that the tides of both thefe oppofite hemifpheroides pafs fucceffively through the meridian of each place in 12 hours; that the northern countries partake more of the north tides, and the fouthern of the fouth tides; and therefore, out of the equator the tides of each day are alternatively greater and fmaller. The greateft tides happen 3 hours after the moon's paffage through the meridian, when this planet declines from the equinoctial towards the zenith, and the
moon

moon changing its declination the tides will be smaller.

The greatest difference between the tides of the same day must happen at the time of the solstices, especially, when the ascending node of the moon is at the beginning of *Aries*. It is also found by experience, that in the winter the morning tides are greater than the evening ones; and in the summer, the evening tides are greater than the morning ones, at *Plymouth*, by about a foot, and at *Bristol* 15 inches.

The other opinions of philosophers concerning the cause and effects of the flux and reflux of the sea, are almost all reduced to those which I have just related; we have therefore thought it necessary to examine what those are which agree with experience, and which are those that are contrary thereto.

As to what *Possidonius* relates, heh as very well distinguished the three motions of the tides which follow the periods of the days, months, and years; but he supposes that the tides are greater towards the solstices than toward the equinoxes, which is not conformable to experience. Perhaps they had observed the tides in the summer solstice when the moon was very near the earth, in which case the sea must have risen to a great height, which had given room to conjecture that the greatest tides always happen in the solstices, and the smallest in the equinoxes. In short, *Strabo*, who relates the opinion of *Possidonius*, says, that this author having been at *Cadiz*, in the temple of *Hercules*, about the summer solstice, had not observed any extraordinary tides in the full moon, but that about the new moon in the river *Bætis*, or *Quadalquiver*, there happened so great an

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overflowing of the waters, that the level of the ground of the light-houfe, and the rampart of the port of *Cadiz* had been covered to the height of 10 cubits.

*Pliny*'s opinion concerning the tides of the equinoxes and folftices, feems more agreeable to the obfervations, as he affirms, that the greateft tides happen in the equinoxes, and the fmalleft in the folftices ; but he advances that the tides are yet greater in the autumnal equinox than in the vernal, which we have not been able to difcover by the obfervations. What is moft remarkable is, that he has found that the tides are fmaller when the moon receeds from the earth, than when it approaches it ; and its force acts nearer, which agrees with our obfervations. He alfo obferves, that in the fpace of eight years, after 100 revolutions of the moon, there are the fame principles of motion of the tides, and the fame augmentations obferved, which has a great deal of relation to the motion of the moon's *apogeum*, which in eight or 9 years returns to the fame point of the Zodiack after 118 revolutions of the moon, which we may confequently take for the period of the principal inequalities that are obferved in the tides. He adds, laftly, that the effects produced by the motions of the celeftial bodies are not fo foon felt upon the earth, as they are perceived by the eye, which agrees perfectly with our obfervations.

With regard to *Galileo*, who pretends that the principal caufe of the *phœnomena* obferved in the flux and reflux of the fea, muft be attributed to the motion of the earth, it would be difficult to reconcile his opinion with the obfervations. He allows himfelf, that according to his principles, there muft be only one flux and reflux in 24 hours, and that

that if it fometimes happens oftener, it proceeds from feveral accidents, as the depth of the water, the directions of the coafts of the fea, &c. But if it was fo, how fhould we be perfuaded that thefe accidental caufes, which vary in fo many different manners, according to the different places, fhould caufe fo regular an effect, that inftead of one flux and reflux in 24 hours, there fhould be two fluxes and refluxes in 24 hours, 48 minutes? An effect which is known to every body, and which we have obferved in all the ports of *France*, that are upon the ocean. where, notwithftanding the various depth of the water, and the different directions of the coafts, the tide regularly takes up 6 hours and about $\frac{1}{4}$ in rifing, and as much in falling.

Befides the periods of the flux and reflux of the fea, which we obferve every day, the caufe of which *Galileo* attributes to the motion of the earth, he finds, that according to his fyftem, there ought to be in the tides a period regulated according to the revolution of the moon with regard to the fun; the earth having, as he conjectures, a greater degree of velocity in the conjunctions than in the oppofitions. To this may be anfwered, that thefe different degrees of velocity, which he attributes to the earth in the new and full moons, were not known till now by the aftronomers. But tho' this effect, which perhaps is not fenfible enough to be obferved by aftronomical obfervations, fhould be enough to make an impreffion upon the fea as *Galileo* fuppofes, it would follow from thence that the tides which happen in the conjunctions would be different from thofe obferved in the oppofitions, and that thofe of the quadratures would be the moft uniform; which does not agree with the

obfervations by which it has been found, that the greateft tides happen equally in the conjunctions and oppofitions where they are pretty uniform ; and that the fmalleft are obferved in the quadradratures, where they are fubject to more irregularities. Nor does the reafon which *Galileo* relates for the annual periods of the tides feem to agree with the obfervations ; for in the folftices, the daily motion of the earth being made in the fame direction as the annual one, it would feem that the compofition of thefe two motions fhould caufe greater tides than in the equinoxes, where the directions of both thefe motions are inclined to one another ; and yet on the contrary we obferve, that the tides are greater in the eqninoxes than in the folftices.

The different degrees of velocity of the annual motion of the earth when it is in its *Perihelion*, or *Aphelion*, muft alfo caufe according to this opinion, a very great alteration in the tides ; neverthelefs we do not obferve any confiderable variation in the tides of the winter folftice, when the earth moves with more velocity than in the fummer folftice, when it moves flower.

*Defcartes*'s opinion concerning the caufe of the flux and reflux of the fea feems more agreeable to our obfervations ; for it is eafy to imagine, that all the celeftial bodies making by their motion fome impreffion upon one another, the earth is obliged to give way on the oppofite fide to the moon ; fo that the waters of the fea are compreffed according to the direction of the moon to the earth, and forced to flow back on both. fides to the diftance of 90 degrees, which makes the high tide.

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The reason which he gives why the tides are greater in the syzygies than in the quadratures, is a consequence of his system, in which he supposes, that the lesser axis of the vortex of the earth, which is elliptical, is always pointed toward the sun; so that the moon is nearer the earth in the syzygies than in the quadratures. But this does not always agree with the astronomical observations ; for it is true, that the moon being in the syzygies, and at the same time in its *perigeum*, is nearer the earth than in any other phase ; but we cannot conclude from hence, that the lesser axis of the vortex of the earth which carries the moon, is always directed toward the sun ; for it often happens that the moon is nearer the earth in the quadratures than in the syzygies ; and yet we always observe, that in the quadratures the tides are less than in the syzygies.

We cannot therefore attribute the cause of the great tides in the new and full moons to the proximity of the moon to the earth, and that of the small tides in the quadratures to its distance ; and this has given us room to conjecture that the sun as well as the moon concurs in producing the height of the tides, altho' its effect was not so considerable as that of the moon ; that in the syzygies both these causes acting according to the same direction, the tides must be greater than toward the quadratures when the sun acted in a direction perpendicular to that of the moon.

*Kepler*, and after him Sir *Isaac Newton*, have judged, that the sun as well as the moon contributes to the height of the tides, with this difference, that as we suppose the tides are produced by the pressure of the sun and moon upon the ce-

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leftial matter which furrounds the earth, they have attributed this effect to the bodies of the fun and moon, which attract the waters of the fea by a vertue almoft like that of the loadftone. Thefe two hypothefes, tho' very different in their principles, feem to be equally able to give a reafon for all the *phænomena* which are obferved in the tides. It is true, that according to the fyftem of the preffure, the fea muft be low in the places where it fhould be high according to the fyftem of the attraction ; but as in the new and full moons the high-water happens in feveral places at different hours of the day, before and after noon, it is not eafy to determine to which of thefe caufes we ought to attribute the flux and reflux of the fea. It is therefore more proper before we embrace any fyftem to be affured of a great number of obfervations, which we have hitherto done, and have had an opportunity of continuing by a new journal of obfervations upon the tides made at *Breft* during the years 1712, and 1713.

This journal begins the 13th of *July*, 1712, where the preceding had finifhed, and has been continued to *March* 1713.

In this interval, which is about nine months, they have obferved the tides of 18 both new and full moons, among which are found thofe of the autumnal equinox, the winter folftice, and the vernal equinox.

The tide which happened the fooneft was obferved *Feb.* 24, 1713, at 3 hours, 6 minutes, in the morning, the new moon being marked that day in the *Connoiffance des Temps*, at 10 hours, 50 minutes, at night. That which happened the lateft was obferved *Dec.* 13, at 4 hours, 27 minutes, ½, the full moon being marked that day at

1 hour, 3 minutes, in the morning. The difference between thefe two obfervations, which is 1 hour, 24 minutes ½, may be partly corrected, by fuppofing as in the preceding memoirs, the mean time of the full moon at *Breft*, at 3 hours, 45 minutes, and making ufe of the common equation of 2 minutes for an hour, which muft be added to the mean time, or fubtract from it, according as it retards or anticipates with regard to the time of the new or full moon. For we fhall find, that the 24th of *Feb.* 1713, the day of the new moon, and of the greateft acceleration of the tide, the high tide ought to have happened at 3 hours, 7 minutes, within a minute near what it was obferved at ; and that the 13th of *Dec.* the day of the full moon, and of the greateft retardation, the height of the tide ought to have happened at 1 hour, 14 minutes ½, within near 12 minutes ½, of what was obferved.

The obfervation of the 24th of *February* is marked for the greateft acceleration in the fpace of almoft two years, and is diftant from the mean time of the high tide 39 minutes, which by the equation prefcribed, are reduced to one minute ; and the obfervation of *December* 13, is that where the greateft retardation was found in the fame fpace of time, and is 42 minutes diftant from the mean time, which by our equation are reduced to 12′ ½, which fhews the necefity of ufing this equation, and the advantage that may be drawn from it, for knowing more certainly the time of high-water, the days of the new and full moon.

As to the time of high-water in the quadratures, it is fubject to great inequalities, which however will be partly corrected, by fuppofing the mean time of the high tide at *Breft*, on the day of the

quadratures, at 8ʰ 57′ as has been done already, and uſing the common equation of 2′ ½ in an hour inſtead of that of 2′, which we ſuppoſe in the new and full moons.

They have been obliged in the quadratures to make uſe of this horary equation of two minutes and a half, becauſe they have obſerved, that one day with another, the tides retard much more toward the quadratures, than toward the new and full moons ; and for this reaſon

In the new and full moons, the preſſure is greater than toward the quadratures, and conſequently the effect, produced upon the waters of the ocean which are in the high water, take up leſs time to communicate themſelves towards the coaſts, than in the quadratures where the preſſure of the moon being much leſs, its effort employs much more time to communicate itſelf toward the coaſts, and cauſes a retardation in the tides, which makes an effect like the waves of the ſea, which are greater, and acquire a greater velocity, the greater the force is which acts them.

We have already obſerved, that the ſea takes up more time in falling than in riſing. This is confirmed by theſe new obſervations ; and it ſeems, that we might attribute the cauſe of it to this ; that the effort which obliges the waters to riſe, drives them with violence, and conſequently with a great deal of velocity toward the coaſts, from whence they afterwards retire by their own weight with leſs velocity than they were raiſed.

With regard to the tides which happen twice in the ſame day, every twelve hours, they muſt continually change in height, ſince in each month they are greateſt one or two days after the new and full moons ; that they afterwards diminiſh

continually

continually till a day or two after the quadratures; that they afterwards rise again, and so on successively; but besides this inequality of height, which they have observed at all times, it also meets with others. For we often observe, that from the quadratures to the new and full moons, the evening tide which ought to be greater than that of the morning, because the tides increase from day to day, is however sometimes several inches less in the morning than at night; and on the contrary, in other circumstances, from the new and full moons to the quadratures, they find the evening tide greater than that of the morning, tho' it ought to be less, because the tides from one day to another continually diminish.

This inequality of height in the tides has been, by Sir *Isaac Newton*'s account, observed at *Plymouth*, and at *Bristol*, by Mess. *Coleprefs* and *Sturmius*, who have observed that in the winter, the morning tide was greater than the evening one; and that in the summer, it was less than the evening one.

Perhaps they made these observations about the new and full moons which happen near the solstices, when there is in effect, almost always this variety of height in the tides which succeed one another every twelve hours.

For example, the 19th of *June*, 1712, the day of the full moon, the morning tide was observed at *Brest* 17 feet 1 inch, less by a foot and an inch than that of the evening, which was found to be 18 feet 2 inches.

The 2d of *July*, the day after the new moon, the morning tide was observed 14 feet 10 inches 5 lines, less by 9 inches 2 lines than that of the even-

evening, which was found to be 15 feet, 7 inches 8 lines.

On the contrary, the 13th of *December* 1712, the day of the full moon, the morning tide was obferved to be 16 feet 7 inches, greater by 4 inches 4 lines, than that of the evening, which was found to be 16 feet 2 inches and 8 lines.

The 29th of the fame month, the day of the new moon, the morning tide was obferved to be 18 feet 10 inches, greater by 7 inches than that of the evening, which was 18 feet 3 inches.

It appears therefore, by thefe obfervations, as by many others, which would be too long to relate, that about the new and full moons, the fummer tides are lefs in the morning than at night; and the winter tides are lefs at night than in the morning, which may eafily be ac-counted for, if we fuppofe, conformable to our hypothefes, that the tides are almoft of equal height in the parts of the earth directly oppofite to one another.

In fummer, in the new moon, this planet paffes about noon with the fun through our meridian, with a northern declination, and confequently its greateft force muft be felt in the northern coun-tries of our hemifphere, and in the fouthern coun-tries of the other hemifphere, which are directly oppofite to us. About twelve hours after, toward midnight, the moon paffes throngh the meridian in the oppofite hemifphere, with a like declination; and confequently its greateft force muft be percei-ved in the northern countries of the other hemi-fphere, and in the fouthern countries of our hemifphere, which are directly oppofite to it : the effort or preffure of the moon in the fum-mer new moons, is therefore greater at noon in

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the northern countries, where we live, than in the
fouthern countries ; and on the contrary, this pref-
fure is greater at midnight in the fouthern than
in the northern countries ; from whence it fol-
lows, that the height of the tide being proportioned
to the different efforts or preffures of the moon,
the evening tide immediately after noon muft be
greater in the new moons of the fummer, than
the tide which happens after midnight.

In the full moons which happen alfo in the
fame feafon, the moon paffes over our meridian
at midnight, with a fouthern declination, and
confequently the morning tide, which immediately
follows, muft be lefs than that of the afternoon,
the moon paffing at noon over the meridian of
a place which has the northern part of the earth
for its oppofite, where the greateft evening tide
muft happen.

On the contrary, in the winter new moons, this
planet paffes with the fun through the meridian
with a fouthern declination, and confequently the
preffure which it occafions in the northern countries
muft not be then fo great as thofe which it
produces in the fouthern countries, and the evening
tide, which immediately follows, muft be lefs than
that of the morning, the moon paffing at mid-
night through the meridian of a place, which
has its oppofite in the northern part of the earth,
where the greateft morning tide muft happen. In
the winter full moons, the moon paffes at mid-
night through the meridian with a northern decli-
nation, and confequently the morning tide which
follows immediately, muft be greater than that of
the evening, the moon paffing at noon through
the meridian of a place which has its oppo-
fite in the fouthern part of the earth, where the
greateft evening tide muft happen.

It appears therefore by this reasoning, that in the new and full moons, the summer tides must be less in the morning than at night ; and that the greatest tide must happen at night. That on the contrary, in the winter, the tides are less at night than in the morning ; and that the greatest tide must happen in the morning.

We observe only that the difference of height between the morning tide, and evening tide, is greater in summer than in winter ; which must in effect happen, because the evening tides, which in summer are greater than those of the morning, by the reasons just related, are also increased in height by the augmentation which is made in the tides every twelve hours, from one or two days after the quadratures, to one or two days after the new or full moon ; whereas in winter, the evening tide, which is less than the morning one, is increased in height, because the tides which increase in the new and full moons, are greater at night than in the morning ; which causes a less difference in the height of the tides of the same day in winter than in summer.

There are therefore two causes, which concur to the variation of height which is observed in the tide, which happen in one day every twelve hours; one, which is produced by the continual augmentation or diminution which happen between the new and full moons, and the quadratures ; and the other, which we must attribute to the different height of the moon upon the horizon ; according to which its declination is more to the north or south. This last cause commonly prevails above the other; when the moon is in the northern or southern signs, but it is hardly sensible in the new or full moons of the equinox,

when

when the moon paffes through the meridian with very little declination.

This augmentation or diminution in the tides every twelve hours, as we have juft obferved in the new and full moons which happen about the folftices, muft be obferved alfo in the quadratures which happen about the equinoxes.

In the vernal equinox, the moon is in its firft quarter in the northern figns, and paffes through the meridian at fix at night, with a northern declination, and confequently the evening tide, which follows its paffage through the meridian, muft be greater than that of the morning. In the third quarter, the moon paffes through at 6 in the morning, with a fouthern declination, and confequently the morning tide muft be lefs than the evening one. Thus in the quadratures, which happen about the vernal equinox, the tides are lefs in the morning than at night, and the leaft muft happen in the morning.

On the contrary, about the autumnal equinox, the moon in its firft quarter is in the fouthern figns, and paffes through the meridian at fix at night, with a fouthern declination, and confequently the evening tide, which follows its paffage through the meridian, muft be lefs than that of the morning. In the third quarter, the moon paffes through the meridian at 6 in the morning, with a northern declination, and confequently the morning tide, which follows its paffage through the meridian, muft be greater than that of the evening.

Thus in the quadratures which happen about the autumnal equinox, the morning tides are greater than thofe of the evening, and the leaft tides muft happen in the evening.

This reafoning agrees pretty well with the obfervations for the 6th of *September* 1711;

the

the day of the leaft tide of the autumnal equi-
nox, the evening tide was obferved to be 10
feet 3 inches, lefs by 7 inches than the morn-
ing tide which was 10 feet 10 inches. The
23d of *September* 1712, the day of the leaft
tide of the autumnal equinox, the evening
tide was alfo obferved to be ten feet eight
inches four lines, lefs by nine inches eight
lines, than the morning tide which was eleven
feet 6 inches.

On the contrary, the 16th of *March* 1712,
the day of the leaft tide of the vernal equi-
nox, the morning tide was obferved to be 10
feet 10 inches, lefs by 3 inches than that of
the evening; and the 20th of *March* 1713,
the day of the leaft tide of the laft quarter,
the morning tide was obferved at 11 feet
7 inches, lefs by nine inches than that of the
evening.

We have obferved in the preceding memoirs,
that the various diftances of the moon from the
earth caufe a very great variety in the height of
the tides. This is confirmed by thefe laft obfer-
vations, for the 28th of *Dec.* 1712, the day of
the full moon, the diftance of this planet from the
earth, being 936 parts, the radius of which is
1,000; that is to fay, the moon being very near
its *perigeum*, they obferved, *Dec.* the 30th, in the
morning, the day of the greateft tide, the height
of the full fea to be 19 feet, 2 inches, above the
fixed point, and that of the low-water to be 1
foot, 8 inches, below this point, fo that the fea
had rifen that day to the height of 20 feet, 10
inches.

The 11th of *Jan.* following, the day of the full
moon, this planet's diftance from the earth being
1064

1064, that is the moon being very near its *apogeum*, they obferved the 15th of *Jan.* in the morning, the day of the greateft tide, the height of the full fea to be 17 feet, 5 inches; and that of the next low-water to be 1 foot; fo that the fea had only rifen 16 feet, 5 inches, that day, lefs by 4 feet, 5 inches, than in the preceding obfervation when the moon was near its *perigeum*.

It muft be obferved, that in the **new moon** *perigeum* of the 28th of *Dec.* 1712, its declination was 23 degrees to the fouth, very ciftant from the equinoctial, and confequently its preffure upon the earth muft not be fo great as when the moon being almoft at equal diftance from the earth, is at the fame time nearer the equator.

In effect we find, that the 24th of *Feb.* 1713, the day of the new moon, its diftance from the earth being 953, that is, near its *perigeum*, and its declination 5 degrees fouth near the equator, the height of the full fea was obferved *Feb.* 26, in the morning to be 21 feet, 2 inches, which is the greateft height that they have obferved at *Breft* in almoft two years. The low-water following was obferved to be 1 foot, 3 inches, below the fixed point, fo that the tide rofe that day to the height of 22 feet, 5 inches.

The 12th of *March* following, the day of the full moon, its diftance from the earth being 1,032, pretty near its *apogeum*, and its fouthern declination one degree, that is, near the equator. They obferved the 13th of *March* following, the day of the greateft tide, the height of the full fea to be 18 feet, 2 inches, and that of the low-water 0 feet, 0 inches; fo that the elevation of the fea that day was only 18 feet, 2 inches, lefs by 4 feet, 3 inches, than in the preceding obfervation,

vation, when the moon was near its *perigeum*, but 1 foot, 9 inches, greater than in the obfervation of *Jan*. 11, 1713, before related, when the moon being near its *apogeum*, its northern declination was 20°.

As to the fmall tides which follow the quadratures, we find alfo by thefe laft obfervations that their heights are proportioned to the different diftances of the moon from the earth. For example, *Sept*. 23, 1712, the day of the leaft tide which followed the 3d quarter, the moon's diftance from the earth being 1,063 very near its *apogeum*, the height of the full fea at night was obferved to be 10 feet, 8 inches, 8 lines, and that of low-water 5 feet, 10 inches, fo that the fea rofe that day only 4 feet, 10 inches, 8 lines. The 7th of *Oct*. following, the day of the leaft tide which followed the 1ft quarter, the moon's diftance from the earth being 976 near its *perigeum*, the height of the full fea was obferved at night to be 12 feet, 10 inches, and that of the morning 3 feet, 6 inches, fo that the elevation of the tide was that day 9 feet, 4 inches, greater by 4 feet, 5 inches, 4 lines, than in the preceding obfervation, when the moon was near its *apogeum*.

The moon's northern declination was *Sept*. 22, 1712, the day of the laft quarter, 24° ½, and confequently the fmalleft tide following muft be very low as was obferved, having been found the 23d at night to be 10 feet, 8 inches, 8 lines, lower by 2 feet, than the 26th of *Dec*. when the diftance of the moon from the earth being 1,036 near the *apogeum*, and its fouthern declination 5 degrees, the height of the tide was found to be 12 feet, 8 inches, 8 lines.

Befides,

Besides the variations in the height of the tides which refult from the different diftances of the moon from the earth, and its different declination with regard to the equinoctial, there muft alfo according to our hypothefis be fome that are caufed by the different diftance of the fun from the earth, and by its different declination, we have already obferved that the tides of the new and full moons were greater than toward the equinoxes, when the fun has no declination, than toward the folftices, when it has 23 degrees, 29 minutes, and it is probable that the fun which is then in conjunction, and in oppofition with the moon, concurs with it to the different heights which are obferved.

With regard to the diftance of the fun from the earth, as it is lefs about the winter folftice when the fun is juft at its perigeum, than at the fummer folftice when it is near its *apogeum*, the tides muft be greater in winter than in fummer, all things being equal, as in effect it is obferved. For *July* 30, 1711, the day of the full moon, its diftance from the earth being 960, and its declination 25 degrees, 29 minutes, the fun being alfo in its *apogeum*, they obferved *July* 1ft at night the height of the greateft tide to be 17 feet 10 inches. The 8th of *January* following, the day of the new moon the moon's diftance from the earth being 951, and its declination 23 degrees, almoft the fame as the 30th of *July*, the fun being then near its *perigeum*, they obferved *January* 10th in the morning, the height of the greateft tide to be 19 feet 10 inches, higher by 2 feet than in the preceding obfervation, when the fun was in its *apogeum*. The 19th of *June* following, the moon's diftance from the earth being 936, and its fouthern declination 24 degrees 50 minutes, the fun being then

I

near its *apogeum*, the height of the greatest tide was observed *June* 21 at night, to be 18 feet 4 inches, less by 1 foot 6 inches than in the preceding observation. Lastly, the 28th of *December* 1712, the sun being in its *perigeum*, the moon's distance from the earth being 936, and its southern declination 23 degrees, the height of the greatest tide was observed the 30th of *December* to be 19 feet 2 inches, greater by 10 inches than the 19th of *June*, when the sun was near its *apogeum*, and the moon at almost equal distance from the earth.

It results therefore from these observations, that there are four causes which contribute to the different heights that are observed in the tides. The first depends upon the different situations of the moon, with regard to the sun, and produces the variations that are observed in the height of the tides, from the new and full moons to the quadratures. The second is produced by the different distances of the moon from the earth, the tides being greater, when the moon is near its *perigeum*, than when it is near its *apogeum*. The third is produced also by the different distances of the sun from the earth, the tides being greater when the sun is in its *apogeum*, than when it is in its *perigeum*. Lastly, the fourth depends on the distance of the moon from the equinoctial, the tide being less when the moon has a great declination than when it is near the equator. This last cause produces also the variations that are observed in the tides which happen in the same day. These variations must be perceived differently in different parts of the earth; they must be null in the countries that are under the equinoctial line; but they are very sensible in the northern and southern countries, according as the declination of the moon is more to the north or south.

VII.

VII. *A description of a portable machine, proper to support the glasses of very great foci; presented to the academy by M. Bianchini, by M. de Reaumur*[*].

The number of observations, with which M. *Bianchini* has enriched our memoirs, have sufficiently informed the publick of his ability and attention in observing the heavens; but his zeal for astronomy does not end there, for knowing better than any body how far this science has been carried since they have known how to work the great glasses, he has thought of rendering the use of them easy. Meff. *Huygens* and *Cassini* did a great deal in shewing us that we might use glasses with greater *foci* without tubes; this was removing a confiderable difficulty by informing us that we might do without instruments, which it was almost impossible to construct, and which it was not easy to make use of. Notwithstanding this fine discovery there still remained many difficulties: for placing these glasses, there must be wooden towers erected such as there is in the observatory; or several solid beams raised upon the ground: it must have large spaces of ground; and besides all these inconveniences, it was necessary to employ several persons and different machines to change the direction of the object-glass, according as the star should change its place; the expences which this required were above the fortune and zeal of many private persons.

This made M. *Bianchini* think that nothing would be more proper to multiply the number of observations, or which is the same thing, to perfect

[*] July 29, 1713.

aftronomy, than a machine which had the follow-
ing qualities: 1ft, that it fhould fuftain the object
glafs very high, altho' light. 2d, that the height
might be eafily altered according to the different
elevations of the planets above the horizon. 3d,
that it fhould be folid and firm, without its being
neceffary to make ufe of nails to fix it, or finking
beams into the earth. From the preceding qua-
lities it was alfo effential that there fhould refult
two others; that the whole machine might be
eafily carried about, and coft but little.

He fought for this machine, which did not ap-
pear eafy to be found, and defired M. *Chiarelli*, a
prieft of *Vincentia*, famous in *Italy* for optical
works, to feek for it in concert with him; *Galileo*,
who has done fo much for the fciences, was alfo
of fervice to them on this occafion. M. *Bianchini*
knowing what this celebrated author has demonf-
trated upon the force of a hollow cylinder, did not
doubt but that he might contrive a fupport for the
object glafs high, folid, and at the fame time light,
by making ufe of hollow cylinders, that is, by
caufing tubes of different diameters to be made
which fhould fhut into one another like thofe of
the telefcopes; the ufe of telefcopes fo familiar to
M. *Bianchani* had conducted him fo far. He had
already a fupport high, light, and of which the
height might eafily be varied; two of the quali-
ties effential to the machine fought for: there on-
ly remained to find a folid and convenient man-
ner of raifing this high fupport perpendicular to
the furface of the earth. This is what M. *Chiarelli*
has very ingeniouſly executed, and has given the
machine all the advantage we could wifh, as will
be feen by the defcription that we are going to give
of it.

A

A hexagonal tube * AB about four feet and a half high, ferves fometimes for a cafe and fometimes for a bafe or fupport to fix other tubes: The breadth of each of the faces of this great tube is pretty near two inches and $\frac{1}{2}$; it is compofed of fix fmall planks either glued or nailed together; the perfection of the machine confifts in thefe planks being thin and of a light wood.

The fecond tube BC is only different from the firft in its fize; it muft enter conveniently into the other but not wave in it, it is longer than that which receives it, tho' its diameter is lefs, by which means it may more eafily be taken out, or which is ftill better the upper end of each tube has a little rim, a fort of little collar, which cannot enter into the tube that ferves for a cafe to it; in the fecond tube there is inclofed a third tube CD, in the fame manner as the fecond is inclofed in the firft, and fo on.

That the tube which ferves fometimes for a bafe and fometimes for a cafe to all the reft may be fupported perpendicularly to the horizon, there are three feet HHH, which like three buttreffes make an acute angle with the horizon, and are fupported againft three of the faces of the tube. The manner in which the feet fuftain it is very ingenious; a little hexagonal tube I, which is only a few inches high, like a fort of ring furrounds part of the great tube between its aperture and the middle; it can rife or fall freely, but is always pretty near within the bounds that we have given it; for the more convenient explaining ourfelves we fhall call it a ring.

To this ring are faftened with hinges three wooden tringles HHH; thefe tringles are equal between themfelves, and of the fame length with

* Plate 6, Fig. 13

the

the great tube or a little longer ; each of them are faſtened over againſt a different face; their breadth is alſo almoſt equal to that of one of the faces of the ſame tube. Theſe three tringles are the three feet of the machine : as they are held by hinges ; it is eaſily imagined that by placing the ring, to which they are joined, between the middle and the upper end of the great tube, and giving it an almoſt equal inclination, they ſupport the great tube on three ſides, and hold it in a vertical poſition.

But as there might be ſome danger of one of the feet ſlipping, this has been remedied by faſtening with a hinge to the extremity of each foot a little tringle KKK of the ſame breadth with the foot, and as long as $\frac{2}{3}$ or thereabout of the great tube; this tringle is joined alſo by its other extremity with a ring L, like that to which the feet are faſtened ; it is needleſs to ſay that this ſecond ring alſo ſurrounds the great tube, that it riſes and falls freely, and that it is placed near the lower end of the tube, when we would raiſe the machine perpendicularly ; it is evident that in this laſt diſpoſition the three lower tringles hinder the three feet from parting.

The manner in which theſe feet and the tringles which hold them are joined together, ſhew that if we raiſe the upper and lower ring along the great tube, we at the ſame time oblige the feet and the tringles to lay upon the tube, the whole machine then takes up little room, as appears in the figure MILNN, and is ſo light that a man may conveniently carry it under one arm.

When we would make uſe of it, we begin by laying it upon the ground, or putting it in

a

a very inclined pofition; we then draw each tube out of the cafe that holds it as far as is thought neceffary, and keep it in the place where we would have it remain in a manner equally plain and convenient; for this purpofe M. *Bianchini* has thought of making ufe of very thin wedges of wood, thefe wedges are eafily taken out and as eafily put in again ; the advantage of this is that we may raife or lower the machine when it is prepared, according as the elevation of the ftar requires ; as it is light there is not much difficulty to put it in a vertical pofition, and to keep it in that pofition we need only unfold the feet.

It might perhaps be feared that its lightnefs would diminifh its fteadinefs, and this would be a well grounded fear, if we could not eafily remedy this inconvenience. In all places where ftones are found, it is eafy to give the machine all neceffary fteadinefs ; by putting fome upon the tringles or traverfes which hold the feet, thus we load it with weights that we are not obliged to carry about.

That the great tube which ferves for a bafe to all the reft, may keep fteady in the two rings that fupport it, it is neceffary to pierce three holes in the thicknefs of the upper ring ; putting a fcrew in each hole, and we need only fcrew them to adjuft the tube.

If we apprehend that the point of the fcrews may in time pierce the wood of the tube, we may cover this tube with a band of very thin iron or copper, in the place where the ring turns when the machine is raifed. By making a little notch quite round it, we may there fix the band, and it will not flip above the reft of the tube.

It is proper alfo to make a rim at the lower extremity of this great tube, or to drive fome nails

into

into it, to prevent the lower ring from flipping off the tube when the machine is carried from one place to another ready mounted.

We shall not stay to explain at length how the object glass is placed at the top of this machine, it may be done different ways; but that of M. *Bianchini* is convenient. This object glass is generally lodged in a cylindrical tube O, which is not above 7 or 8 inches long; but this tube has a sort of tail P above a foot long. This tail is only a wooden gutter; a sort of tenon Q of thin wood is glued against the outer surface of the tube, toward the middle of the length of this tube there is a hole in this tenon; another tenon of wood like the former is set at 7 or 8 inches distance, and is glued under the tail P of the tube.

In the last of the hexagonal tubes of the machine, there is put a piece of wood in which is cut a vertical notch; this noth receives the first of the two tenons that are fastened to the tube of the object glass; this tenon is held by the means of a screw.

To the second of the pieces that is fixed to the tube, or rather to the tail of the tube of the object glass, is fastened a small packthread. This packthread is at least as long as the focus of the glasses; it serves to mark the distance that the eye-glass must be fixed at.

This eye-glass is in a tube V, like that of the object-glass, I mean that has a sort of gutter which makes it a tail X, under this tail the second end of the packthread Y is fastened.

To sustain the tube of the eye-glass, M. *Bianchini* makes use also of a support Za composed of tubes that shut into one another. He has made these square, and might have made them of any

other

other figure, for that is very arbitrary, and there does not feem to be any effential reafon for making the tubes that ferve for a fupport to the object glafs of a hexagonal figure rather than any other.

The fupport of the eye-glafs is compofed of three tubes; the laft, that is to fay the lowermoft *a*, is terminated by a notch; in this notch there is put a little plank *b*, and this plank is held in the notch by a fcrew: the thicknefs of this plank ferves for a foot to the fupport of the eye-glafs: this is not a very folid foot, it is however a reft, becaufe this fupport is put in an inclined pofition, in fuch a manner that it makes an obtufe angle with that part of the horizon that is between it and the fupport of the object-glafs. The packthread, one end of which is faftened to the tube of the object-glafs, and the other end to the tube of the eye-glafs, holds the fupport of the eye-glafs in this fituation.

As to the reft, it is eafy to fee of what height all the tubes together of this laft fupport ought to be: it muft be fuch that a man ftanding upright, or fetting, may apply his eye near the glafs.

All the glaffes with their tubes, and the fupport of the object-glafs, may be inclofed in a fmall and light box; fo that the fame man, who under one arm carries the fupport of the object-glafs, may with the other, carry the box that holds all the reft of the *apparatus*: thus an obferver who has not near him ground enough, or ground proper for obferving, carries when he pleafes into the fields what is neceffary for it, he is in a ftate of chufing the moft convenient grounds for his obfervatory; he may do it alone; but it is however right to have a perfon with him to deprefs or elevate fome of the tubes of the great fupport, according

cording as the ftar afcends or defcends, though in cafe of neceffity he might do the whole alone.

A high wind would not be a favourable time to obferve with this machine; the wind however makes lefs impreffion on it than one would imagine.

## VIII. *A hiftory of an extraordinary fleeping,* *by M.* Imbert.

Sleep is the moft melancholy and humbling ftate of man; in health it has bounds that nature has the art of prolonging often by habit, or conftitution. Among animals, the dormoufe, and marmottee, fleep fix months in the year without awaking. A fleeper of this fort is a rare example, the hiftory of which has feemed to me to be worthy of the enquiry of a philofopher that is a curious obferver.

A man of about 45 years of age, of a dry and robuft conftitution, whofe name was *Tally*, who drove the *Rouen* coach, and was a carpenter by trade, fell into the diforder I am fpeaking of by the following accident. He had quarreled with a carpenter for whom he had worked; they were parted juft as they were going to fight, and each went his own way. A little while after, our fick perfon heard that his adverfary had fallen from a building, and was killed. This fatal news feized him, he threw himfelf with his face upon the ground, and his fpirits and fenfes grew drowfy infenfibly. The 26th of *April* 1713, he was carried to *la Charité*, where he remained till the 27th of *Aug.* of the fame year, that is, 4 months. The 2 firft months he gave no fign of voluntary motion or fenfation; his eyes were fhut day and night; he often moved his eye-lids; his

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respiration was always free and easy, his pulse was small, and slow, but equal ; if you put one of his arms in any situation it remained there, (a disease that is called a catalepsy); but it was not the same with the rest of his body ; they made him swallow some spoonfuls of wine to support him ; and this was his only nourishment during this time ; he therefore became lean, dry, and emaciated ; a very different state from that which he was in before. M. *Burette*, under whose hands he was, at first made use of the most powerful assistances of art, bleeding in the arm, the foot, the neck, emetics, purgatives, blisters, leaches, and volatiles, and this without being able to procure any other relief to him than that of talking very sensibly to his family, and the clergy, for an entire day, after which he fell again into his sleeping. The two last months of his stay at *la Charité*, he by intervals gave some marks of sensation, sometimes pressing his wife's hand, and at other times by melancholy complainings ; but this would happen when they had been several days without purging him. From this time he ceased to do all under him, being careful to turn himself to the edge of the bed, where a waxed cloth was put on purpose, and not to do any thing till he found himself there, and then he did his occasions, and returned to his place ; he began also to take broths, pottage, and other sustinence, keeping still his first inclinations, a great thirst for wine. He never made any signs that he wanted any thing. At the times appointed for his meals, they touched his lips with their fingers ; at this signal he opened his mouth without opening his eyes, and swallowed what was given him ; he then lay still expecting patiently a second notice. They shaved

him regularly, but he was all the time like a corps fet upright. If he was taken up after dinner, they found him in his chair with his eyes fhut in the fame pofture that they had left him. A week before he went out of *la charité*, they threw him naked into cold water to furprife him. This remedy furprifed him effectually ; he opened his eyes, looked fteadfaftly, but did not fpeak at all. In this condition his wife carried him home, where he is at prefent : they give him no medicine ; he fpeaks fenfibly enough, and mends every day.

Here is a ftumbling block for a philofophical reafoner ; being always impatient to get the maftery of nature in her moft hidden defigns, he fees, admires, and fearches, and yet difcovers nothing. I fhall venture however to propofe to the company as conjectures, fome reflections that I have made upon fo fingular a hiftory. That I may reprefent them in order, I fhall firft examine how grief may produce this kind of fleep ; in the fecond place I explain the different alteration which have happened to it ; in the laft place I feek for examples that may have fome relation to it. In the firft propofition two things are to be confidered ; upon what fleep depends, and the manner in which grief acts. There are many caufes that produce fleep in general : in the brain, obftruction in the glands, compreffion or relaxation; from hence commonly proceed apoplexies, and lethargies : in the blood, impoverifhing of the fpirits ; and from hence proceeds the indifpenfable neceffity for man to fleep to repair them ; fpirits too much incumbered by the grofs parts ; and hence proceeds the difpofition always near to the fleeping difeafes. Such was the ftate of our

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patient before he fell. A carpenter by profeſſion, and a ſot by inclination, qualities which commonly furniſh thick blood, the active principles of which are hard to be diſengaged; reaſon proves it, and experience confirms it every day. This being ſuppoſed, it remains to examine the manner in which grief acts. Grief is a diſeaſe of the mind, one of the moſt terrible and moſt fatal, rage, deſpair, fear, revenge, and melancholy, are its uſual effects. What diſorders do not paſſions of this nature produce in the machine! Some precipitate the motions of the ſpirits without order, whence phrenſies ariſe, and an infinite number of acute diſeaſes, others retard the courſe of it, and therefore produce hypochondrical affections, and the greateſt part of chronical diſeaſes. The grief of our ſleeper is of the laſt ſort: at the news of his enemy being killed, he is ſeized with terror, and fills himſelf with melancholy ideas; fear and ſadneſs retain his ſpirits in the brain, his blood naturally thick and deprived, if I may uſe the expreſſion, of the *primum mobile*, thickens more and more, its parts draw cloſer, hang together and entangle the ſpirits; hours of reſt are no longer ſufficient, but whole months are requiſite to ſeparate a quantity of it neceſſary for waking. In this reſpect I am not afraid of comparing him to the marmotte: being thus aſleep he is its true image. This animal, heavy by its natural conſtitution and dull, abounds with fat during the time of its ſleeping; it takes no nouriſhment in its ſix months ſleep; the ſpirits diſengage themſelves inſenſibly by the motion alone of the circulation of the blood, and the reſpiration which it preſerves: at the end of this time it awakes without any help; the ſix months that it is awake, it

A a a 2                                                eats

eats reafonably, exhaufts but little, its blood be-
comes of the fame fort, and it fleeps again. Per-
haps from the fame principles, and the fame rea-
foning, we might explain in a more probable
manner the changes which happened to our patient
during his fleeping ; the two firft months his fleep
was profound ; his blood in all appearance had
acquired the quality of the blood of the marmotte,
the other two months without opening his eyes, or
fpeaking ; he however by intervals, gave fome
figns of fenfe.   By the exact diet that he obferved,
the fpirits difengaged themfelves, and a greater
quantity of them were feparated ; the marmotte
requires fix months, nature has fo ordered in
forming it ; here it is an accident, and may be re-
paired in lefs time.   We have a proof of it, and
our patient grows better every day ; it now re-
mains to feek for the examples which may have
relation to it.   Neither the ancient nor modern au-
thors furnifh us with any. M. *Homberg* read to the
company in the year 1707, the extract of a *Dutch*
letter printed at *Goude*, containing the hiftory of
an extraordinary lethargy ; it deferves to be fet
down here as a parallel ; grief was the occafion of
it ; the fleeping was preceded by a melancholy
affection of three months.   For the length
of time the *Dutch* fleeper exceeds the *French* one,
he flept fix months fucceffively without interrup-
tion, and during this time gave no figns of vo-
luntary motion, nor of fenfe ; at the end of fix
months he awaked and difcourfed with every bo-
dy ; and 24 hours after he returned to fleep again ;
perhaps he may be afleep ftill, for we have not
the reft of this hiftory. The carpenter in queftion,
in four months ficknefs, had only two of real fleep,
but the cataleptic accident, the figns which he
<div align="right">pre-</div>

preserved of a man asleep, those which he gave of a man awake, the effects that followed the bath of cold-water, are so many rare particularities, which render the fact worthy of the attention of the most learned philosophers and physicians; my design was to have entered into a particular explication of all these accidents, but the fear of being troublesome makes me defer it to another dissertation.

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# A
# TABLE

## OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCXIV.

## In the HISTORY.

## In the MEMOIRS.

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# ABRIDGMENT

## OF THE

PHILOSOPHICAL DISCOVERIES and OB-
SERVATIONS in the HISTORY of the
ROYAL ACADEMY of SCIENCES at
*Paris*, for the year 1714.

I. On *the paſſage of air and water through
certain bodies.*

WE are commonly perſuaded, that water,
though more groſs than air, penetrates
certain bodies, as paper, which air does not. But
perhaps alſo it penetrates them for this very reaſon,
that it is more groſs ; that is, that it has power
to make paſſages which air cannot ; perhaps alſo
air penetrates the ſame bodies as water, but with-
out being perceived ; for it is very dangerous in
philoſophy to ſuppoſe facts which are not ſtrictly
proved, and we are generally miſtaken in them.

M. *de Reaumur* has thought of a very ſimple
and infallible method of being ſure of theſe facts
and all others of the ſame nature. The quickſil-
ver keeps ſuſpended in the barometer, only be-
cauſe the tube is ſo exactly cloſed at its upper ex-
tremity, that no air can enter. If any ſhould en-
ter, the quickſilver would immediately fall in
proportion to what got in, and if it entered by
little and little, the quickſilver would alſo fall by
degrees, till at laſt it was quite upon a level. If
inſtead of air it was water that entered into the ba-
<div align="right">rometer</div>

rometer, the quickfilver would ftill fall according to the weight of this quantity of water. We know how much the quickfilver ought to fall for a determinate quantity of both, and reciprocally by the quantity of its falling we know how much of either got in. But if both entered, the water being vifible, we know how much air muft have got in to make the total effect. It is evident that we muft always confider how much the quick-filver ought to fall independantly on the entrance of the air, or water, by the fole variation which would happen to the barometer at the time of thefe experiments.

This being fuppofed, we need only clofe, as M. *de Reaumur* has done, that extremity of the barometer, which is to be uppermoft, with the matter which is to be examined whether it is penetrable by air; if the quickfilver finks in the tube independantly on the diminution of the weight of the atmofphere, we fhall be fure that fome air has got in, which muft have penetrated the fubftance which ftopped the end of the tube. And this fort of proof has this advantage, that the air acting only by its gravity againft what ftops the tube, and this gravity being a known force, equal to 28 inches of quickfilver, we know it is this entire force which at firft caufes the air to enter, fince the top of the tube is perfectly void, and inclofes no air that refifts the outer air. If the air continues to enter, we know by the quantity which is already entered, and which is known by the falling of the quickfilver, how much the force which drove it is diminifhed, and if the quickfilver at laft comes to a level, we fee that the leaft force is capable of making the air pafs through the body under examination. We fee alfo, which is very confiderable, what are the different times,

*in*

in which this force continually decreasing has oc-casion to act according to its different decreasings. But if the question is about water instead of air, or about both together, M. *de Reaumur* has con-trived to make with a certain composition, impe-netrable by air, a little rim raised above the tube, by means of which he has a little vessel, into which he pours the quantity of water desired, and the same reasonings follow.

I shall now mention the result of the experi-ments which he has made upon these principles.

Air passes through paper even the thickest, but less quick.

Let the impelling force be ever so little, it passes but more slowly.

If paper be ever so little wetted, the air does not pass through, it begins to pass again as soon as the paper is dry ; If we would have it continue wet, it must be rubbed with oil.

Air passes pretty freely through old parchment. It does not pass when it is wet.

It is known that water penetrates the bladders of several animals from without inwards, and not from within outwards. Air seldom penetrates a hog's bladder, when its inner surface is exposed to it; and when it does penetrate it, it is with an ex-treme slowness, especially when it is impelled but by a small force.

Air does not penetrate a hog's bladder by its outward surface, but water penetrates it, tho' pretty slowly.

There passes with the water a very small quanti-ty of air, which proves both that water may pass where air does not, and that when it does pass, it is by opening itself passages, thro' which air takes the advantage of accompanying it.

Water

Water, tho' impelled only by a little force, penetrates the inner furface of the bladder, which it does not penetrate in the living animal, but this is becaufe it is not then impelled by any force, fince thefe two furfaces are equally preffed by the inward and outward air : Thence M. *de Reaumur* concludes, that membranes of our body, which in their natural ftate are not penetrable by certain liquors, will become fo when an extraordinary rarefaction of air fhall caufe them to be lefs compreffed by one of their furfaces than by the other. We fee by this example, that M. *de Reaumur's* experiments, which might naturally be ufeful in the arts, will be fo alfo in medicine, and this unforefeen ufe may give us reafon to expect more.

## II. *Of fome extraordinary effects of thunder.*

The Chevalier *de Louville,* being at *Nevers,* obferved fome remarkable effects of a violent ftorm of thunder there ; a tree of the park of the caftle, was fplit into three at the top of the trunk, and had three furrows of unequal bignefs made in the wood, juft as if three musket balls had been fhot from the top of the tree towards the root ; the tree was barked on one fide from about the middle quite to the bottom ; tho' it was crooked, yet the three ftrokes followed the bendings exactly, fliding continually between the wood and the bark, both in the upper part of the trunk which was ftill covered with bark, and in the lower part which was covered but on one fide ; but what was moft remarkable was, that the wood was not blackened at all, and had no fign of burning.

Upon this, the chevalier *de Louville* explains how thunder can have great effects without burning ; it is certain in the firft place, that it has great

ones

ones upon animals, and that when it falls pretty near them, the fulphurous vapour alone is fufficient to take away their breath and kill them, without the appearance of any hurt or bruife upon their whole body. But as for the tree, there muft be another caufe; M. *de Louville* thinks that when the thunder falls from fuch a height, that its flame is diffipated before it can reach the earth, the air being violently driven by the impetuous motion of this flame, and confequently condenfed in an extraordinary manner, becomes a fort of hard body, of which the fhock muft have a great deal of force.

Hence alfo he explains another clap of thunder, the effects of which he faw at *Nevers*, the fame day. There was a faggot upon the two dogs laid in the chimney in order to be kindled, the thunder fell down the chimney and broke the faggot into a hundred thoufand bits, without fetting fire to it, or fo much as blackening it; probably the funnel of the chimney confining the courfe of the air, had alfo augmented the impetuofity of it.

On this occafion it was faid, that the kindled matter which forms the thunder, may come out of the cloud in a fmall quantity, and afterwards find in the air a great deal of matter of the fame nature and kindle it, for then the air is extremely loaded with fulphurous exhalations. Perhaps this is partly the reafon why lightning crinkles, for it is feeking in the air for a nourifhment which is irregularly difperfed.

III.

### III. *Of some petrified shells found at a considerable distance from the sea.*

M *de Lagny* has seen in *Poitou* some petrified shells very well preserved, which were found 8 or 10 feet deep in the ground, upon some hills 10 or 12 leagues from the sea ; there were among these shells several *cornua ammonis*. He saw also a field quite covered with oister shells as large as plates.

### IV. *Of the* American *rat, or* mus alpinus.

M. *Sarrasin* physician at *Quebec*, from whom we have had an exact and curious history of the beaver in the memoirs of * 1704, has sent one of the *American rat*, which is very like that which **Mr.** *Ray* has described under the name of *mus alpinus.* It also so much resembles the beaver, that **M.** *Sarrasin*, who knows them very well, says he should take it at first sight for a beaver of three or four months. That which he dissected weighed four pounds; we shall not enter into the anatomical particulars.

This rat is of the class of animals that gnaw. In *March*, when the snow that always falls abundantly in *North America*, is not entirely melted, it goes out and lives upon pieces of wood that it breaks; after the melting of the snow, it commonly lives upon the roots of nettles, afterwards upon the stalks and leaves of that plant, and in summer upon strawberries and raspberries, its nourishment grows continually more delicate. A little while after its going abroad it thinks of multiplying its species ; they go freely together 'till autumn, and at the beginning of winter they se-

* Vol. II. Page 181 of this abridgment.

parate, and take up their lodging each in its hole, in some hollow tree, without any provisions. This is what the savages relate, who according to M. *Sarrasin*, observe the nature of animals pretty well, which is the only part of philosophy with which they are acquainted.

To make the long time which the *American rat* must live without nourishment more probable, M. *Sarrasin* relates that at *Quebec*, he chained a bear fast under some planks covered with snow; from *November*, and that in *April* when the snow was melted it was found there alive and well.

### V. *On the effect of the siphon* in vacuo.

A bent tube or *siphon* being put into a vessel full of water, by one of its branches, which I call the first, and the other consequently the second, it is plain that the pressure of the exterior air upon the water of the vessel must not make it rise in the branch, because this branch is filled with an air that presses the water which answers to it, and opposes itself to its elevation with a force equal to that of the exterior air. The air contained in the second branch has also the same action as that of the first, and opposes itself in like manner to the elevation of the water; but if we go to suck at the end of the second branch, we draw the air of both and diminish the quantity of it; and consequently the exterior air, which presses upon the water of the vessel becomes the strongest, and makes the water rise in the first branch, from whence it passes into the second.

If we leave off sucking, we must in order to know what will happen, determine the length of the second branch with regard to that of the first. The air, which tends to enter again into the

the fecond branch, and from thence into the firft, has in this tendency or action the whole force of the weight of the atmofphere, *minus* that of the column of water contained in the fecond branch, which acts againft it. On the other fide, the exterior air which elevates the water in the firft branch, has the whole force of the weight of the atmofphere, *minus* that of the column of water contained in the firft branch, the elevation of which exhaufts a part of its force. Thus here are two forces equal in themfelves, but both weakened by the circumftances and acting one againft another; if they are equally weakened, that is, if the two branches of the fyphon are of the fame length, there will be an *equilibrium*; and confequently as foon as we leave fucking, the water will ceafe rifing in the firft branch, and going out by the fecond. Much more will this effect happen, if the fecond branch is the fhorteft, and by the contrary reafon the water will continue to go out by the fecond branch, if that is the longeft, as it always is in the fyphons, which are intended only for this ufe.

The weight of the air therefore is the caufe of the effect of the fyphons, and no philofopher difputes it; the fyphons alfo being put into motion in the free air yield the water more flowly in the pneumatic machine, in proportion as we pump out the air, and at laft quite ftop when the air is pumped out as much as it can be. If we place them again in the free air they do not begin to flow, unlefs we fuck them anew, and it evidently muft be fo, becaufe they are in the fame cafe as if they had never flowed.

M. *Homberg* however has obferved that certain fyphons, which had ftopped in the *vacuum*, had begun again to flow of themfelves, as foon as

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they were set again in the free air; these have a very small diameter, as about ⅓ of a line: now what force must that be which puts them in motion again as soon as they are in the free air ?

When they were first there they emitted the water drop by drop, and at the distances of about 2 seconds, whereas the others of a greater diameter emit it in continued threads of an equal diameter to that of the second branch. This difference comes from the very small syphons being full of water as soon as they are wetted, in their inner surface, a drop of water which wets a small part of this surface, joins itself to the drop of water which is overagainst it, and this by a certain viscidity, which naturalists acknowledge to be in water. When these syphons are in the free air, and are once moistened by the water, which has passed through, that their motion may be continued, the weight of the air must surmount not only the weight of the water which it has raised, but the viscidity of it also, which is performed only by a certain quantity of water collected, and consequently with a certain time, and thence it comes that these syphons flow only drop by drop and at several times. Each drop that comes out partly falls, because it is impelled by the weight of the upper drops. When we put these syphons *in vacuo*, not only the weight of the air acts continually less and less, and at last acts no longer, but also the air contained in the water is extended, because it is no longer pressed by the outward air ; it disengages itself from within the water, and forms great bubbles, which interrupt the *series* of the drops of water, with which the two branches were moistened and filled ; and those which are at the extremity of the second, have no longer a

suffi-

fufficient weight, and are no longer fufficiently preffed by the others to make them fall. If we replace the fyphons in the free air, the air which was extended is obliged to refume its firft bulk, the drops of water which it no longer keeps feparate fall again, the upper upon the lower ones, and the fyphon begins to flow again as faft as it is wetted, but always drop by drop, and always more flowly, and does not ceafe 'till its fecond branch is dry, at leaft to a certain point.

It follows from this explication, that if water was without air inclofed in its interftices a very fmall fyphon would continue to flow *in vacuo*, as long as it was wet. This is alfo what M. *Homberg* has experimented with water freed from air, whether becaufe it had been made to boil well, or becaufe it had been put into the pneumatic machine, and this *phænomenon*, which feems at firft fight fo contrary to the fyftem of the weight of the air, agrees perfectly with it, and is a neceffary confequence of the fpring of the air ftretched by its gravity.

It is eafy to forefee, that if for the experiment of the capillary fyphons, we make ufe of liquors which contain more air, or air which difengages itfelf more eafily, fuch as fermented liquors, the fyphons will ftop fooner in the *vacuum*, alfo, all the reft being equal, they muft ftop fooner in winter than in fummer, for in winter the air is more difpofed to difengage itfelf, fince in frozen liquors it is full of great bubbles ; we fhall judge alfo by this experiment, that fat liquors, as oil or milk contain lefs air, or air more engaged, for with thefe liquors the fyphons do not ftop in the *vacuum* in any time whatfoever.

VI. *On the greatest possible perfection of ma-
chines moved by animals.*

Mechanicks can do nothing more ingenious and
more useful at the same time, than to determine
how far they may be useful, and in what bounds
all the advantages which they promise are contained.
All possible machines are moved either by animals
or by fluids, which are made to work instead of
animals ; those of the last sort have already been
examined by M. *Parent*, and now he examines
those of the first.

An effect of a machine can never be greater than
the natural and simple effect of the power which
moves the machine. Thus if the natural effect of
the force of a man is to raise 24 pound, by going
1000 toises in an hour, and of a horse to raise
170 pound, by going 1800 toises in an hour, a
machine moved by a man or by a horse could ne-
ver do more let it be compounded with ever so
much art, and it would even be much less because
of the inevitable frictions, but we do not consider
them here. The effect of the machine moved by
a man will therefore never be more than the pro-
duct of 24 pounds, by a 1000 toises of velocity
in an hour, or the product of 24 by 1000, in
what manner soever this product is formed by the
weight and by its velocity; for it is always the
same quantity of motion, and thence it follows,
that a man going 1000 toises in an hour, may
raise a weight of 24000 pounds, provided this
weight goes but one toise in the same time, and
it is the same with all the other infinite ways
by which the product 24000 may be formed.
The mechanical effect therefore has necessarily for
a limit the natural effect of the power which moves
the

the machine, and indeed it is impoffible to draw
a new force from nothing. If we would have a
man going 1000 toifes in an hour raife 24 pound,
it is beft not to ufe any machine, but if we would
have him raife more than 24 pound, there muft
be one, which preferving to the man his natural
velocity, diminifhes that of the weight in propor-
tion as it is bigger : the whole comes to this, the
different arms of the lever, by which either the
power or the weight act in machines, only regulate
their velocities, and always reprefent them geo-
metrically.

When therefore we have a machine moved by
animals, which raifes a weight, its effect being the
product of the weight by the velocity which the
machine gives it, there is nothing more eafy than
to compare this effect with the natural effect of the
animals, and thereby to fee how much it is lefs, for
it is always fo becaufe of the frictions ; the more
the machinal effect fhall approach to the natural,
the more perfect will the machine be.

If animals draw one or more boats, the obftacle
which they have to overcome is the refiftance of
the water, the greatnefs of this refiftance depends,
1ft, on the greatnefs of the furface which pufhes
the water before it. 2d. on the velocity of this fur-
face with regard to that of the water, which is cal-
led the *refpective* velocity of the furface : If it is
moved the fame way with the water, its refpective
velocity is the excefs of its velocity above that of
the water; if it is contrary to the water, its ref-
pective velocity is the fum of its velocity, and of
that of the water ; if the water has no velocity,
as that of a pond, then the refpective velocity of
the furface is its proper and abfolute velocity.
Now it muft be remembred, that becaufe we are
confidering a moved fluid, we ought to take the

fquare

square of the respective velocity. 3d. The resis-
tance of the water depends on its weight, or on
its mass, for it is plain that the same surface mo-
ved in the air with the same respective velocity,
would find less resistance than in water.

The obstacle which animals have here to over-
come is therefore the product of these 3 magni-
tudes, the weight or mass of water, the surface
moved in the water, and the square of its respective
velocity: M. *Parent* concludes from several ex-
periments made by skilful mathematicians, that
the water of the *Seine* striking against a surface
of a foot square, with a velocity of a foot in a
second, has a force of 22 ounces; it now remains
to know, what shall be the surface that the boats
shall present to the water, the difficulty is that
their surfaces are crooked, and may be differently
crooked in each. But M. *Parent* shews a very
easy way of making them all equal to a plain sur-
face, I mean the surfaces immerged in the water,
which must push it before them or bear its resis-
tance; we need only put upon the end of a post
in the middle of the river a fixt pulley, over which
passes a cord, one end of which is fastened to the
boats that we would draw, and the other to a great
flat table, plunged in the water by degrees, 'till
it receives a sufficient impression to be driven ac-
cording to the stream, and thereby obliges the
boats to move in order to rise again; it is cer-
tain the part of the table immerged in the water,
will be a surface equal to those of all the boats to-
gether, which have the resistance of the water to
surmount, there will be as many times 22 ounces
of force in the water, as square feet in this sur-
face.

We shall therefore have the resistance of the wa-
ter expressed in a certain number of pounds, acting
with

with a velocity of a foot in a fecond againft a furface of a certain number of fquare feet. Here is what the effort of the animals muft equal in the ftate of *equilibrium*, here is what they muft fuftain out of the *equilibrium*, here is the refiftance they muft furmount to draw the boats, and as they will imprefs a certain velocity upon them, which will augment the refiftance of the water, which the animals will have to overcome, it will be the weight of 22 ounces taken as many times as there are fquare feet in the flat furface of the experiment, and multiplied by the fquare of the refpective velocity of the boats; and it is plain, that this product can never be greater than the natural effort of the animals, as we have determined.

If it is a running water, its velocity enters into the expreffion of the refpective velocity of the boats, whether they go up or down, and as this velocity is determined, that of the *Seine*, for example, to be one foot in a fecond, there is a determinate quantity which enters into the refiftance of the water, and confequently requires that a certain part of the force of the animals fhould be determined alfo. All the reft almoft is free, that is, we may vary the force of the animals as we will, and the oppofite charge, which is the fum of the furfaces of the boats immerged in the water. If the charge remaining the fame the force of the animals diminifhes, or which is the fame thing, their number, for we muft fuppofe that they always go with the fame velocity, or if the force of the animals remaining the fame, we augment the immerged furfaces of the boats, either by augmenting their number or loading them more, it is plain that in thefe two cafes the boats will go more flowly. But if the water is ftill, the refpective velocity of the boats being no more than their proper velocity,

and

and the determined velocity of the water making no longer a part of it, then all is free, and we may draw as great a number of boats, or as much loaded as we will, with as small a force as we will, on condition that the boats shall go very slowly, and this is only what is continually found in all mechanicks.

If the animals which draw the boats are applied to a machine fixed upon the shoar, as M. *Parent* suppofes, this machine will furnish the arms of a lever, one of which will belong to the animals, and the other to the oppofite charge or load, the proportion of which will reprefent that of the velocity of the animals, to the velocity of the boats.

But if instead of a machine fixt upon the shoar, we make ufe of a machine carried upon the boats themfelves, which can be only a double mill fastened on the outfide to the two fides of the firft boat which will draw the reft, then the water acting againft the fails, or vanns or floats of each mill, and obliging the cord drawn by the animals to turn about a roller, will imprefs on the boats a motion added to that which the animals imprefs on it.

In all cafes whatfoever, M. *Parent*'s theory gives him a fure mean to find any magnitude whatfoever, that enters into the moving force or the load oppofed, when the other magnitudes shall be given or known. It is only a calculation, but it fometimes requires art and delicacy in the application, and thence it comes to have its particular beauty.

VII. *Of a new theory of the working of veffels.*

This year there appeared a new book of M. *Bernoulli*, intitled an effay towards a new theory

of

of the working of veffels, the firft and only one that has hitherto come out of his hands ; for he has contented himfelf with difperfing either in our memoirs, or in the *Leipfic* acts, different detached pieces, each of which is equal in value to many a great book. The occafion of this work was the *theory of the working of veffels* by the Chevalier *Renau*, printed in 1689, when it appeared, M. *Huygens* had a confiderable difficulty with M. *Renau* on a fundamental point. As thefe fubjects do not belong to pure geometry, but depend on a very nice mixture of geometry and phyficks, and befides as this was quite new, and as M. *Renau* was the firft who ventured to touch upon it, it was no wonder, that there fhould ftill remain fome difficulties to clear up, or even that the geometricians fhould be divided about it : and indeed they were divided, for fome were for M. *Huygens*, others for M. *Renau*, and among thefe laft was M. *Bernoulli*, who not having feen M. *Renau*'s book, judged of the difpute only as it had been explained to him by the Marquis *de l'Hopital* ; a long time afterwards he faw the book and altered his opinion, but he ftill found himfelf in oppofition to M. *Renau* on another important point, which M. *Huygens* had not taken the pains to confider, or elfe had been convinced of it, and as the alteration of thefe two points make a different theory from that of M. *Renau*, much lefs fimple indeed and more embarraffing, but according to M. *Bernoulli* neceffary, and by its great very difficulty more inciting to a great geometrician, he refolved to make a complete work of it. We fhall give an account of it, as if it was the only one that had been compofed on this fubject, and without entring into either fide of the controverted points, we fhall only give fome preliminary or general informations
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which will facilitate the underftanding of the book, for in order to a more large difcuffion, the book itfelf ought to be confulted.

Suppofe a veffel at reft with its fail, which is fuppofed to be flat, and continues always fo notwithftanding the action of the wind; this veffel being impelled by the wind in a firft inftant, takes at prefent, becaufe of its great bulk, no more than an almoft infinitely fmall velocity, and confequently the water refifts it but very little. The wind blows again in a fecond inftant, and impreffes on the veffel a new velocity, which being added to the firft makes an accelerated velocity, and the water makes more refiftance to this greater velocity.

Laftly, the velocity of the veffel is continually accelerated from one inftant to another, and the refiftance of the water continually augments alfo, till this refiftance becomes of an equal force to the augmentation of the velocity of the veffel; then if the wind and water were to be fuddenly annihilated, and the veffel confequently found in the *vacuum*, it would go to infinite according to a right line, with an uniform velocity equal to the laft degree of acceleration, which it received from the wind in the laft inftant. But this laft degree, to which the refiftance of the water is equal, being acquired, the veffel is not in the *vacuum*, the action of the wind and the refiftance of the water always fubfift; however, becaufe this action and refiftance are become equal, they mutually deftroy one another, and the veffel is in the fame cafe as if it was in the *vacuum*; it will go therefore thenceforward with an uniform velocity, and the continual action of the wind upon the fail will only deftroy the continual refiftance of the water. It is in this ftate of uniform velocity that the motion of the veffel is confidered.

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That the motion may be effectively uniform, the wind muft no longer accelerate it; and for this it muft meet the veffel, not as flying before it, for it would accelerate its motion ; but as being at reft, and that the wind may always meet the veffel as being at reft, tho' it really flies; the velocity of the wind muft be as infinite with relation to that of the veffel. This is alfo what M. *Bernoulli* fuppofes in his whole theory. It is true, that when a veffel makes 3 leagues in an hour, whilft the wind makes 5, the fuppofition is very far from the truth ; but M. *Bernoulli* reafons fafely upon this fuppofition, the known errors of fuppofition are not errors in geometry.

The force of the wind, which drives the veffel with an uniform velocity, and the refiftance of the water being equal, and deftroying each other, thefe two forces muft act one againft the other in the fame right line, for otherwife they would produce a common effect, and not deftroy one another.

The force with which the wind acts upon a fail always fuppofed flat, depends upon three things, or magnitude, by which it is produced, as that of every fluid which ftrikes a plain furface.

1. On the magnitude of the furface.
2. On the angle of incidence of the fluid on the furface:
3. On the velocity of the fluid.

The 1ft point is clear.

As for the 2d, it is clear that a fluid, which ftrikes a furface obliquely, ftrikes it only according to the degree of perpendicularity in its direction; that is, according to the fine of the angle of incidence, and confequently the impulfe is

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so much stronger as the sine is greater, or the oblique impulse less oblique. Besides, in proportion as the incidence is more oblique, a less quantity of the fluid strikes the surface, and we shall see it plainly by conceiving the incidence infinitely oblique or parallel to the surface, for then the surface is not struck by any quantity of the fluid ; and in the opposite case, which is that of the perpendicular incidence, it is so thro' the whole possible quantity of the fluid. It is very easy to prove, that the different quantities of water, which answer to the different incidences, are as the sines of the angles of incidence. Whence it follows, that the forces of the different impulses are as the squares of these sines.

For the 3d point, every body knows that the different impulses of a fluid, moved with different velocities, are as the squares of these velocities ; because a fluid moved with more velocity, strikes both with more force, and in the same time with a much greater number of parts, and that this greater number is in the same *ratio* with a greater velocity.

Therefore the force of the wind upon the sail is a product of the surface of the sail, by the square of its sine of incidence, and by the square of its velocity. If we suppose the wind always the same in different cases, its velocity is no longer to be considered. It will be the same with the sail always supposed the same.

The resistance of a water which is not running, as that of the sea, to a vessel which moves, is the same as the impulse of the same water if it was running, against the same vessel at rest. Thus the resistance of the water is regulated by the impulse

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pulfe of the wind upon the fail, though with a a great difference which muft be obferved.

A veffel has a crooked furface, compofed confequently of an infinite number of plain furfaces infinitely fmall, differently inclined to each other, which caufes the incidence of the water to be different from each of them ; whereas the incidence of the wind is the fame upon the whole fail, always fuppofed flat. From all the *partial* refiftances of the water to each infinitely fmall furface of the crooked veffel, there is formed a *total* refiftance, which may be alfo called mean ; and it is this refiftance alone that is equal, and directly oppofite to the action of the wind upon the fail.

To be able to exprefs it geometrically, we muft know the curvity of the veffel, and the geometricians plainly perceive, that we fhould then fall into integrations that are often impoffible, and always difficult. M. *Bernoulli* avoids all this difficulty by confidering at firft a veffel, which is only an oblong rectangular parallelipiped, or if you pleafe, a fimple parallelogram, which confequently has but two fides differently ftruck by the water.

Thefe principles being eftablifhed, and thefe fuppofitions made, there are two principle things to be confidered on the motion of the rectangular veffel, its courfe and its velocity.

It is plain at firft, that its courfe, or the direction of its motion, depends on the line, according to which the wind impels the fail. The wind, which in general is fuppofed oblique to the fail, impels it only according to what perpendicularity the fail has in its direction and by no means according to its parallelifm, and confequently the line

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according to which the wind impels the fail, is always perpendicular to it. Thence it follows, that if the fail is always in the fame fituation, or directed according to the fame diameter of the horizon, the wind, tho' it falls upon the fail under different angles, will always impel it according to the fame line, becaufe the perpendicular to the fail will be always the fame, or directed to the fame point of the horizon. Much more will it be fo, if the incidence of the wind is always the fame, for the wind blowing according to a certain determined diameter of the horizon, the equality of incidence determines the fail to be directed according to another diameter of the horizon, which is always the fame. M. *Bernoulli* calls the perpendicular, according to which the wind impels the fail, *the line of the moving force.*

If the veffel was round, the perfect uniformity of its figure would make it find in all its parts an equal refiftance of the water, and confequently it would go according to the line of the moving force, or the perpendicular to the fail. If the veffel being rectangular had infinitely more facility to cleave the water according to the line of its *keel,* which would be a right line drawn thro' its centre parallel to the great fide, than according to the line perpendicular to the keel, it would go according to the line of the keel, and not according to that of the moving force; for it receives from the wind an impreffion of motion, and becaufe of the refiftance of the water fuppofed infinite according to the great fide of the parallelogram, this motion cannot be executed but according to the fmall fide, and confequently will go only according to the line of the keel. But the

fuppofition was chimerical, and the veffel which has more facility to cleave the water by the little fide than by the great one, has not infinitely more of it, and confequently is found in a mean cafe between the two extremes which we have confidered; one of which was that of the equal refiftance of the water, and the other that of its refiftance infinitely unequal. Therefore, if in the 1ft cafe it followed the line of the moving force, and in the 2d that of the keel, it muft in the mean cafe follow a mean line, which will be its true courfe.

In the conftruction of veffels it is endeavoured to give them fuch a curvity, that the greater facility they have to cleave the water by the prow, or according to the line of the keel, than by the fide, may be the greateft poffible, and the intention fhould be, that the courfe be made according to the line of the keel. It is impoffible however, but that the water muft refift the prow as well as the fide, though much lefs, and confequently the line of the courfe turns afide more or lefs from that of the keel, according to the greater or lefs inequality that there is between the refiftance that the water makes to the prow, and that which it makes to the fide, which comes from the different figure of the veffel. The angle which the courfe makes with the keel is called the *Angle of the Derivation*.

It feems at firft, that the magnitude of this angle muft depend only on the greater or lefs inequality of the refiftance of the water, or which comes to the fame thing, on the figure of the veffel. But M. *Bernoulli* makes another confideration to enter into it. The figure of the veffel being determined, the line of the moving force may

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be fuch, that it will drive the veffel more according to the keel, which is the way by which it advances moft eafily, or that on the contrary it will drive it more according to the perpendicular to the keel, which is the leaft favourable way to the motion. Now the more the veffel fhall be impelled the way that it advances moft eafily, the more will its courfe approach to the keel, and on the contrary. And confequently the veffel having always the fame figure, the angle of the derivation will vary according to all the different pofitions that the line of the moving force can have with relation to that of the keel.

It is evident, that if the line of the moving force concurs with that of the keel, there is no derivation, and the courfe is made according to the keel.

When the courfe is as ufual, a mean line between that of the moving force and the keel, this mean line is in the fame cafe, as if according to the theory of compound motions it refulted from the actions of two forces, one of which acts according to the line of the moving force, and the other according to the keel. In this cafe, if thefe 2 forces were expreffed by the two lines which they would endeavour to run over in the fame time, the diagonal of the parallelogram, of which thefe 2 lines would be the fides, would exprefs the refult of their actions, or the lines that the movable ought to run over. We may therefore conceive the veffel as impelled by 2 winds, and for the greater facility, by 2 winds perpendicular to one another, each of which fhould be perpendicular to the fail which is expofed to it. We fuppofe alfo the fails equal. It would feem that the 2 lines, which each wind in particular will make the veffel

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run over in the fame time, being determined in magnitude, the diagonal of the rectangle will be the courfe that the veffel will make in the time determined, but M. *Bernoulli* reafons otherwife. The velocity, which each wind in particular would imprefs on the veffel, is uniform, according to what we have faid, and the refiftance that the water will make to each of thefe velocities, is equal to the force with which the veffel is impelled by each wind. Now here all the magnitudes which enter into the refiftance of the water being equal, except the velocities, the two refiftances of the water, each equal to the force of the correfponding wind, are therefore as the fquares of the velocities, and confequently we ought to confider the veffel as impelled by two forces which are as thefe fquares. The parallelogram, the diagonal of which fhall be the courfe of the veffel, muft not therefore be made under two fides, which are as the velocities, but under 2 fides which are as their fquares, which changes the direction or pofition of the diagonal, or of the courfe.

The courfe of the veffel will be reprefented by this diagonal, but not by its velocity, fince the fides of the parallelogram reprefent not the velocities but their fquares. This diagonal will therefore reprefent the fquare of the velocity of the veffel, whence the velocity will be eafily deduced.

But to examine the velocity yet more clofely, let us confider, that it is fo much the greater as the moving force is greater, and moves the veffel according to a line more favourable to the motion.

The wind and the fail being always fuppofed the fame, the greatnefs of the moving force de-

pends only on the incidence of the wind upon the fail, the nearer this incidence approaches to perpendicularity, the wind has the more force, and the veffel the greater velocity.

But if we fuppofe the incidence of the wind upon the fail always the fame, and confequently the moving force equal, there is ftill one thing which makes the velocity of the veffel vary, the pofition of the fail with regard to the wind being determined and fixed, that of the fail with regard to the keel is not fo, and we may by means of the rudder, put the keel into different fituations with regard to the fail, the courfe is a mean line between that of the moving force; which in the prefent fuppofition is fixt, and that of the keel. We have already feen, in fpeaking of the angle of *derivation*, that according as the line of the moving force was more or lefs approaching to that of the keel, the veffel was impelled in a manner more or lefs favourable to its motion, and this is the fecond condition on which the velocity depends; we fhall now fee this more particularly.

The courfe is a mean line between the line of the moving force and that of the keel, and it changes its direction as we have feen, when the angle of thefe 2 extreme lines changes. Therefore the pofition of the line of the moving force being fixed, if that of the keel changes, the courfe changes alfo. It is on the direction of the courfe that the different incidence of the water on the great fide depends, and on the little one, of the rectangular veffel that is, that according to the different courfe the angles of incidence of the water upon each of the two fides vary between themfelves, and are more or lefs unequal. Thefe two angles taken together always make a right

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one, but the fines of two angles that are equivalent to a right one, do not make a fum equal to that of the fines of 2 other angles, which are alfo equivalent to a right one, and confequently according to the different courfe of the veffel, the fums of the fines of the angles of incidence of the water upon the two fides are different. Now thefe fines or their fquares exprefs the different refiftances of the water to each fide, and confequently the fum of the two *lateral* refiftances of the water varies according to the different courfe, and confequently alfo the velocity of the veffel. It is plain that a lefs velocity anfwers to a greater fum of the lateral refiftances.

The two lateral or partial refiftances of each courfe produce a mean or total refiftance, which it is eafy to calculate. Each of thefe mean refiftances is equal to the force by which the veffel is impelled. Now in the two different courfes, the moving force is the fame, fince the incidence of the wind upon the fail is fuppofed the fame ; therefore the two mean refiftances are equal. and as there neceffarily enters into the expreffion of each, the particular velocity, by which the veffel is impelled according to each courfe, we have the relation of thefe two different velocities to magnitudes, into which there enter only the known fides of the veffel, and the courfes or lines which depend upon it.

From all that has been faid it refults, that there muft be *a greater* for the velocity of the veffel, that is, that the incidence of the wind upon the fail being always the fame, there will be a pofition of the keel with regard to the line of the moving force, fo that the veffel will have a greater velocity in this difpofition than in any other. This

greater velocity, which the calculation gives without difficulty, is found when the diagonal of the rectangular veſſel is in the line of the moving force, and conſequently perpendicular to the ſail; one might be ſurpriſed at firſt, that it is this diagonal, which ſhould be put in this ſituation, and not the keel, that is, the line drawn through the centre of the parallelogram, parallel to the great ſide, for the keel is always the moſt favourable line to the motion. But it is eaſy to ſee that the greater velocity anſwers to the leaſt reſiſtance of the water, and that it is at the point, or at any angle whatſoever of the parallelogram that the water reſiſts the leaſt; what had been called the keel was not ſo in reality, it is the diagonal that is ſo, and the error of the word being diſſipated, there is no longer any paradox.

The line which expreſſes each velocity, is always taken upon the line of the courſe, and as the different velocities of the veſſel for each courſe, or which comes to the ſame thing, for each poſition of the keel, have known relations, we may conceive the infinity of lines which will expreſs the velocities as drawn, and forming by their extremities a curve, which M. *Bernoulli* calls *the determiner of the velocities.* He ſhews how to draw it by points, that is, for all the different poſitions of the keel, the angle of the wind with the ſail being always the ſame.

This being done, we ſee that unleſs the line of the courſe does not touch the curve, which only one courſe can do, it cuts it in two points, and as all the points of this curve determine the velocities, there are therefore, except in one ſingle caſe, two different velocities for one ſingle courſe; and becauſe each point of the curve anſwers to a different

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position of the keel, we may make the fame courfe with 2 different pofitions of the keel, but with 2 different velocities. It is very eafy to fee which of the two is the greateft, and we muft chufe that, and confequently give the keel the pofition which anfwers to it.

There is a confiderable advantage which they often endeavour to procure by the working, and that is *gaining upon the wind*, as a wind which blows from eaft to weft, for example, and falls obliquely upon the fail, impells it only according to a perpendicular, it does not impel the veffel to the weft, and the perpendicular may be fuch as fhall be directed according to the line of the S. E. and confequently its direction will partake of a direction from W. to E. and laftly the impulfe of the wind fuppofed will tend to make it go partly from W. to E. and even againft the wind. Now it is an advantage to go againft the wind, or to gain upon the wind, not only when we would take a courfe, to which it is almoft entirely contrary, but when we would board a veffel, or avoid being boarded by it. M. *Bernoulli*'s curve fhews alfo what is the courfe that we muft hold, or the pofition that we muft give the keel, to gain upon the wind as much as poffible.

If the courfe that we would hold is determined, which is the moft ufual cafe, we find by means of the curve, that 2 different pofitions of the keel are equally proper for it, and we ought to chufe that which gives the greateft velocity.

All that has now been faid fuppofes the angle of incidence of the wind upon the fail conftant, or to fpeak more exactly, the line of the moving force always directed to the fame point of the horizon, which, as we have feen, fubfifts as long as the fail

is directed according to the fame diameter of the horizon, whether it receives the wind under different angles or under the fame; but if the pofition of the fail changes, which makes the direction of the line of the moving force change, or of the perpendicular to the fail, then there would anfwer to this new pofition of the fail another pofition of the keel, with regard thereto, to give to the veffel the greateft poffible velocity in this new hypothefis. And in like manner all the infinitely different pofitions of the fail will have their different pofitions of keel correfponding to a greater velocity.

If we had determined neither a pofition of the fail according to a certain diameter of the horizon, nor a pofition of keel with regard to the fail, and had fought to determine thefe two pofitions together, to have the greateft poffible velocity, this greater velocity would be therefore the greateft among the infinite number of greateft, each of which refults from a certain determinate pofition of fail, and from the moft advantageous pofition of the keel correfponding thereto. The determination of this *greateft of the greateft*, is one of the moft elevated geometrical problems. M. *Bernoulli* refolves it, and is led by this refolution to important remarks, the occafion of which is rare. But we cannot enter either into this inquiry, or thefe reflections. M. *Bernoulli* himfelf prefers another method for the practice ; he calls it mechanical, and yet it depends on a fublime geometry.

If for each different pofition of the fail the determining curve of the velocities was defcribed, there fhould be an infinity of thefe curves, fince there may be an infinity of pofitions of fail infinitely little different, and this infinity of curves, the firft and fecond, the fecond and third, &c. would

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interfect each other fucceffively in points infinitely near. The feries of this infinity of points of inter-fection would form a new curve which would touch all the *determiners of the velocities*, and would contain all their conditions and advantages. M. *Bernoulli* fuppofes it, not exactly, which is impoffible in practice, but fufficiently well de-fcribed; and he fhews how to find by means thereof, what will be the pofition both of the fail and of the keel, to make a courfe propofed with the greateft poffible velocity, or to gain the moft upon the wind.

Hitherto the figure of a parallelepiped or pa-rallelogram, which has been given to the veffel, has been very different from that which is ufually given them. To come ftill nearer, M. *Bernoulli* takes a veffel which has the figure of a rhomb, or lofenge, but the fame principles always fubfift, only the explications of it become more difficult, and the calculations more complicated. Laftly, after having paffed thro' thefe degrees, M. *Ber-noulli* comes to a curvilineal figure. It is in gene-ral any portion of a curve, which has an *axis*, and ordinates.

The curve which reprefents the figure of the veffel cannot be ftruck by the water, but according to the line of the courfe, or which is the fame thing, parallelly to this line. The parallels to the courfe drawn upon all the infinitely fmall fides of the curve are different incidences to it, but each of them ftrikes only according to a perpendicular to a little fide that it ftrikes. Any one of thefe perpendiculars to the curve being drawn or con-ceived to reprefent all the reft, it is compofed of two directions, one of which is perpendicular, and the other parallel to the *axis* of the curve.

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Thence it follows, that each little fide is ftruck or impelled according to two directions, one perpendicular, and the other parallel to this *axis*. If we have the proportion of the fum of all the impulfes perpendicular to the *axis*, to the fum of all the parallel impulfes, a rectangle, of which the two fides fhall have this proportion being defcribed, the diagonal will reprefent the impulfe or mean refiftance of the water, and the pofition of this diagonal will be neceffarily oblique to the *axis* of the curve. We know by all that has been faid, that the pofition of this line of the mean refiftance of the water is always the fame as that of the line of the moving force.

If the incidence of the water upon all the little fides of the curve changes, there are formed two other fums of impulfions or refiftances, perpendicular and parallel to the *axis* of the curve ; thefe fums have another proportion, and confequently the diagonal of the fuppofed parallelogram, or the line of the mean refiftance of the water, changes in magnitude and pofition, and the line of the moving force changes alfo in pofition. Now the incidence of the water on all the fmall fides of the curve neceffarily changes, as foon as the veffel changes its courfe, which is vifible ; therefore the change of the courfe makes the pofition of the moving line change. It is reciprocally plain, that if the pofition of the line of the moving force changes, that of the courfe changes alfo. The pofition of the line of the moving force gives that of the fail, fince it is always perpendicular to it.

Thefe two lines, that of the moving force and that of the courfe being mutually dependent on each other, as to the pofition, one of the two po-

fitions being given, the other therefore may be deduced from it. Let us fuppofe at firft the pofition of the given courfe.

To have that of the line of the moving force, or of the mean refiftance of the water, we muft have the fums of the lateral refiftances of the water, fome perpendicular, the others parallel to the *axis* of the curve, and confequently determine the curve. M. *Bernoulli* fuppofes the figure of the veffel to be formed of two equal circular fegments, having the keel for a common chord, which comes pretty near the ordinary figures. He expreffes algebraically the differentials of the two forts of lateral refiftances, aud happily the fums of thefe are found to be integrable; and confequently he has their proportion, whence all the reft follows. This proportion is pretty compound.

It changes, as we fee it muft do, at each alteration of the courfe, but there are 2 principal caufes, which in fome fort change the fpecies of it. There is always one half of the veffel, which bears the refiftance of the water, whilft the other is intirely covered from it. If the half expofed to the refiftance of the water is one of the two circular fegments entire; or if this half is compofed of the 2 equal halves of the two fegments, it is plain that the refiftance of the water is confiderably different in the 2 extreme cafes, much greater in the firft, becaufe the veffel takes no advantage of its pointed figure to cleave the water; and much lefs in the fecond, becaufe it takes as much advantage of its point as poffible. Now what determines the half of the veffel expofed to the water is the 2 lines parallel to the courfe, the moft diftant from it on both fides that can be drawn through two points of the furface of the veffel. If they pafs through the 2 extremities or points of the veffel,

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it is in the firſt caſe, and preſents to the water only one ſingle circular ſegment ; if they paſs thro' the 2 extremities of the perpendicular drawn thro' the middle of the keel, the veſſel is in the ſecond caſe, and preſents to the water the half of each circular ſegment, or advance of point.

If the 2 parallels to the courſe paſs thro' the 2 points of the veſſel, or extremities of the keel, and at the ſame time are each of them tangents of their ſegment or circular arch, it follows from thence, that the arch which they touch, or the infinitely ſmall ſide, which is one part of them at the point of contact, makes with the keel the ſame angle as the courſe. But if the courſe, and conſequently the two parallels come to incline more upon the keel, and conſequently to make with it a leſs angle than the fixed angle, and conſiſting of each circular arch with the ſame keel, then the two parallels become tangents of the 2 circular arches in other points, and one of the points of the veſſel begins to be contained in the part expoſed to the water, and is more and more ſo, till at laſt the line of the courſe being infinitely inclined to the keel, or concurring with it, the two parallels are tangents of the veſſel at the two extremities of the perpendicular, which paſſes through the middle of the keel. But if the angle of the courſe with the keel is greater than that of each of the two circular arches with the keel, the two parallels paſs always through the two extremities of the keel, but without being tangents of the veſſel, the part expoſed to the water is always one entire circular ſegment ; and laſtly, the courſe may concur with the perpendicular to the keel. Theſe are the two principal caſes which change the ſpecies

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fpecies of the proportion between the lateral re-
fiftances of the water.

We fhall find the velocities of the veffel of a cir-
cular figure for different courfes, the moving force
being the fame, as we have found thofe of the rect-
angular veffel. It is the fame reafoning.

But if in the veffel of a circular figure, the po-
fition of the line of the moving force was deter-
mined, and the courfe was to be determined, we
might do it ; but by an algebraical calculation, fo
long and fo compound, that M. *Bernoulli* thinks
it more proper to fubftitute tables, wherein the
pofitions of the line of the moving force fhould an-
fwer to all the courfes, and the courfes to them.
Thefe tables fhould alfo contain the velocities.

To bring all this to the utmoft exactnefs, we
fhould determine the figure of the veffel, which
would make it bear the leaft poffible refiftance
from the water. This determination has already
been made for a veffel, the courfe of which fhould
be according to the keel, but it is not in the fup-
pofition of all the other poffible courfes, and as the
refiftance of the water will change in all thefe cour-
fes, there muft be a different figure of a veffel for
each of them: It does not appear therefore, that
geometry can afpire to this enterprife, and we
muft hold to the trials of experience, which will
nearly determine a mean figure, the moft fuitable
to all the courfes.

The pofition of the line of the mean refiftance
of the water with regard to the courfe, or which
comes to the fame thing, with regard to the keel,
being determined,this line neceffarily paffes through
a certain point of the keel. On another fide the
line of the moving force paffes through the middle
of the fail, and what is a neceffary confequence
of it, through the maft to which the fail is faftened,

or,

or, which comes to the fame, through the foot of the maft which always refts upon the keel, or, laftly, through a certain point of the keel. If the courfe is fuch, that the line of the mean refiftance of the water, and that of the moving force pafs only thro' the fame point of the keel, the refiftance of the water, which is equally diftributed on both fides of the line of the mean refiftance, is therefore not e- qually diftributed on both fides of the line of the moving force, and as thefe lines muft however be one right line, the veffel will turn till they are both in this pofition. To hinder this turning, we muft know through what point of the keel, the line of the mean refiftance of the water paffes, and there fix the maft.

M. *Bernoulli* fhews how to determine this point. The refiftance perpendicular to the *axis* of the curve being exprefs'd algebraically and conceived as fo many weights faftened to a lever, which is the *axis*, have a common centre of gravity there- in. In like manner the refiftances parallel to the *axis* have a common centre of gravity upon a perpendicular to the *axis*; two perpendicular lines both drawn through thefe two centres of gravity, interfect each other in a point, which is the centre of gravity of the two fpecies of refiftances; the line of mean refiftance therefore muft pafs through this point, which confequently determines the point where it cuts the keel, and where the maft fhould be fixed.

But as the line of the mean refiftance changes for every courfe, the point where the maft fhould be planted would change alfo, which being im- poffible in practice, we can only find for the maft pretty nearly a mean fituation, the moft com- modious for all the different courfes.

It

It remains only to rectify the fuppofition of the fail always flat, it is very certain that it is not, and that by the action of the wind it takes a certain curvity, thereby the line of the moving force changes, and it is a perpendicular to the curve of the fail, fuch that on both fides the impulfes of the wind on the differently inclined parts of the fail are equal, or which is the fame, that it is its *axis* of *equilibrium*, as the line of the mean refiftance of the water is the *axis* of *equilibrium* of all the particular refiftances. We muft therefore determine the curvity of the fail, or which is the fame problem more fimple, the curvity of a fimple cord, which fhould be in the fame circumftances as the fail, and for this it is neceffary to eftablifh two principles, one of which has been already in this difcourfe.

1ft. A homogeneous and perfectly flexible cord being ftretched, it is equally fo in all its parts, what figure foever it takes by the tenfion; for if we conceived one part to be more ftretched, as it would make an effort to refume its firft ftate, it would draw to it a lefs ftretched part, which would yield to it, and confequently both would come to an equal tenfion.

2. A cord, which in its different parts receives oblique impulfes or preffures, is impelled or preffed only according to the perpendiculars to the points of incidence, and thefe perpendiculars are the fines of the angles of incidence.

When a cord bends becaufe it is drawn by weights or pufhed by a fluid which acts upon it, it is ftretched or impelled at the fame time and by the fame forces, but is equally ftretched and unequally impelled or preffed. And to fee the difference of thefe two actions of tenfion or preffure ftill better, we need only obferve, that a cord or any part

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whatfoever of a cord, is ftretched only fo far as it is drawn according to its length or direction, and that on the contrary it is preffed only fo far as it is preffed perpendicularly to this length.

The tenfions make no alterations by themfelves in the direction of a part of a cord, but the preffures change it, fince they neceffarily impel this part according to a certain perpendicular.

When a cord bends, its only becaufe each of its infinitely fmall parts, which becomes a fide of the curve, receives the action of a power different from that which has acted on the preceding fide, or which fhall act on the following. Thereby each little fide takes a different direction from thofe of the two fides which contain it. If a cord was at the fame time unequally ftretched, and unequally preffed, we ought to conceive fome fide longer, becaufe of the inequality of tenfion, and at the fame time of a direction more or lefs different from that of the neighbouring fide, becaufe of the inequality of preffure. Now I fuppofe it is known, that the greatnefs of the bending of a curve is in a direct *ratio* of the greatnefs of the change of direction from one fide to the other, or of the angle of contingence and in a reverfed *ratio* of the magnitude of the fides into which we conceive it to be divided. Therefore if the cord was unequally ftretched and preffed, it would take a curvity which would be in a *ratio* compounded of the reverfed *ratio* of the tenfions, and of the direct one of the preffures. But the cord is always equally ftretched, therefore its curvity is only in a direct *ratio* of the preffures, and there is the general determination of all thefe forts of curves. As the *radii* of the evolutes are always in a reverfed *ratio* of the curvatures, we muft take thefe curves fo that their *radii* of the e-volute at each point may be in a reverfed *ratio* of

the

the preffure at the fame point. Therefore we have only to determine what fhall be the different fpecies of preffure.

If we confider, as we do here, a fail fwoln by the wind, the preffures will be as the fquares of the fines of the angles of incidence of the wind upon each little fide of the curve. This is plain from all that has been faid; we fuppofe that each thread of wind gives its ftroak to the fail, after which it goes freely out of its cavity; otherwife it would make in this cavity different reflections, which would change the primitive impulfes, and confequently the nature of the curve.

If the fail contained an elaftic fubftance, as air, the fpring of which acts equally every way, it is plain that the preffures being all equal, the curve fought would be a circle.

If we fuppofed a linnen bent by a heavy liquor, which it fhould contain, the preffures would be the heights of the different columns of this liquor.

We muft not forget in this place that M. *Bernoulli* finds again what he had formerly demonftrated, that the curve of the fail is the fame with the *catenaria*, a curve formed by a cord faftened at its two extremities to two fixt points, and bearing at all its points, of which the number is infinite, as many equal weights. It feems that by following the reafonings which have been made, one may eafily prove the identity of the two curves.

The *catenaria* is equally ftretched in all its parts, and confequently it will be the fame as the curve of the fail, if the preffures on both fides follow the fame proportion. The preffures of the *catenaria*, are drawings of weights, all the directions of which are vertical and parallel. The direction of each weight is oblique to each little fide of the *catenaria* and confequently muft be decompounded into

perpen-

perpendicular and parallel to the *axis* of the curve, which is the line drawn from one of the fixt points to the other. The perpendicular to the axis is the only line, according to which the weight draws the fide, it is the fine of the acute angle, which is made by the vertical direction of the weight with the fide, and, if we may. fo fpeak, of the angle of incidence of the weight. The greater this perpendicular to the *axis*, or this fine is in proportion to what parallelifm there is in the direction of the weight, the more it acts or draws with force.

It is eafy to fee, that by taking the curve from one of the fixt points to its middle, the fines of the angles made by the directions of the weights with the fides are continually increafing ; for all the weights being fuppofed equal, their actions or drawings can be only as their diftances from this fixt point, which continually augments, but their actions are as thefe fines, therefore the fines continually augment from a fixed point, where the origin of the curve is, quite to its middle.

This being fuppofed, each fide of the curve is fo much the more drawn, not only as each weight acts by a greater fine, but alfo as each weight acts with more advantage with regard to a neighbouring fide, for to draw the cord downward, it is fufficient for a weight to have any force whatfoever, but to bend it actually, it muft draw it more than another weight draws it, without which all the force poffible would be of no fervice. In a word, there muft be a force in each weight, and an inequality of forces in all, I mean in one half of the curve. Now not only the force of two weights taken feparately and by themfelves will be expreffed by two fines, fuch as we have marked, but their inequality, or proportion of forces will be expreffed by the fame fines; whence it follows, that

the

the drawings of thefe two weights will be as the fquares of thefe fines, which is exactly the proportion of the preffures of the wind in the curve of the fail. Therefore the curve of the fail and the *catenaria* are the fame thing.

In the application of M. *Bernoulli*'s theory to practice, there is a very confiderable advantage, that to determine the pofition of the *axis* of *equilibrium* of the impulfes of the wind, or of the line of the moving force upon the bent fail, we are not obliged to know the nature of the curve of the fail, and to calculate it, which would be very difficult. M. *Bernoulli* demonftrates, that if from the middle of each of the two fides, one above, the other below, which terminates the bent fail, we draw a tangent, the line, which fhall bifect the angle of the concurrence of the tangents, will be the *axis* of *equilibrium* of the impulfes of the wind.

A N

*An Explanation of the Terms of Art used in this Volume, which were not explained at the End of the former Volumes.*

## C.

C *Repusculum* the *Twilight.*

## E.

*Empalement* of a flower is that part which covers the *Petals*, or coloured leaves. In a rose, the five green leaves on the outside of the flower, is the *Empalement.*

## I.

*Iris* is the variegated circle, which surrounds the pupil of the eye.

## L.

*Lamina spiralis* is a thin plate, which divides the *cochlea,* or last cavity of the ear into two parts. That part, which is next the

axis,

*axis*, is bony, but extremly brittle; and that next the outer shell is membranous, appearing to be only made of the auditory nerve.

## M.

*Membrana tympani*, is a thin tranfparent membrane, which clofes the inner extremity of the *meatus auditorius*. It is of an oval figure, ftretched out like the head of a drum; it makes an obtufe angle with the upper and back part of the *meatus*, and an acute one with the lower and fore part.

## S.

*Summit* or *apex* is ufed to exprefs thofe little buttons, which are fuftained by the *chives* or *ftamina* in flowers. They are moft commonly of a yellow colour, and contain a fine golden duft, which is thought to be analogous to the male fperm of animals.

*Syzygia* or *fyzygy*, is ufed to exprefs either the conjunction of a planet with the fun, or its oppofition to it.

## T.

*Terras* of the obfervatory at *Paris* is a platform on the top of that famous ftructure. It is paved with flints laid in mortar, cement, and lime.

*Tympanum* is a pretty large cavity in the ear, fituated behind the *membrana tympani*; it is about 3 or 4 lines deep, as many wide, and between 2 or 3 high. It is lined with a fine membrane, on which there are feveral veins, and arteries. In this cavity are contained 4 fmall bones, called the *malleolus*, the *incus*, the *ftapes*, and the *os orbiculare*.

A

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C.

# CONTINUATION *of the* ADDENDA.

## *In the* MEMOIRS, *of* 1701.

X. *Of the meridian of the royal obfervatory, prolonged to the* Pyreneans ; *by M.* Caffini*.

The journey, that we made this year by order of the king into the fouthern provinces of his kingdom, was to prolong the meridian line of the royal obfervatory, to meafure it quite to the extremities of *France* by geometrical operations ; to divide it into degrees of the circumference of the earth by the obfervations of the ftars ; to examine whether thefe degrees are equal between themfelves as has been fuppofed till this age, or whether they are fenfibly unequal, as feveral modern mathematicians fuppofe ; whether the perpendicular lines marked by the plummet in our geometrical and aftronomical inftruments, are all directed to the fame point, which is taken for the centre of the earth, according to the common *hypothefis*, or whether from different places, which are at a great diftance from each other, they are directed to points fenfibly different, as feveral moderns conjecture.

Thefe laft inquiries ought to have been made before it was undertaken to calculate the whole circuit of the earth by a fmall part. For unlefs thefe

* November 14, 1701.

two laft *hypothefes* of the roundnefs of the earth, and
of the direction of the perpendicular to the fame
centre are well eftablifhed, there is no certainty in
the practice of this' method. Thus *Pliny* had
fome reafon to admire at the boldnefs of the hu-
man mind, in attempting fuch difficult things.
The enterprize alone of examining thefe two *hy-
pothefes* by obfervations is great, and would be
rafh without the protection of fo great a king.
The *hypothefis* of the roundnefs of the earth, as
compofed of continents and feas, that of its de-
tachment from the heavens, and of its fufpenfion
in the air, was firft of all founded upon the obfer-
vation of the apparent motion of all the ftars from
eaft to weft, and on the diverfity of the apparent
conftitution of the heavens, in the voyages made
almoft under the fame meridian towards the fouth
and towards the north. This diverfity compared
with the length of the way gave the firft views of
meafuring the circumference of the earth by the
obfervations of the ftars. The oldeft author now
extant, who has related the particulars of this me-
thod, is *Ariftotle*, who fpeaks of it as a thing already
in practice in his time.

He expreffes himfelf to the following purpofe,
at the end of his fecond book *de cælo*.

" It is manifeft, fays he, by the appearance of
" the ftars, that not only the earth is round, but
" that it is not of any immenfe bignefs; for if we
" travel ever fo little towards the fouth and north,
" the horizon varies, and the vertical ftars, which
" we have over our heads, make a great change,
" and are not all the fame that we fee going to the
" north and fouth. For there are fome which we
" fee in *Egypt*, and near *Cyprus*, which we do not
" fee in the northern countries, and towards the
" north there are fome ftars which we always fee
                                                      " above

" above ground, tho' they fet in the places juſt
" mentioned. Wherefore thoſe who think that
" the ſea, which is at the pillars of *Hercules*, unites
" and makes the ſame ſea with that which is at the
" *Indies*, do not imagine improbabilites.

" The mathematicians, adds he, who endea-
" vour, to calculate the magnitude of the circum-
" ference of the earth, make it amount to 400,000
" *ſtadia*. Whence it is inferred, not only that
" the earth is ſpherical, but that it is not very
" great with regard to the other ſtars.

This muſt have been the language of the authors
of this dimenſion. For *Ariſtotle* far from ſuppoſ-
ing the earth to be a ſtar, in the preceding chapter
refutes the *Pythagoreans* of *Italy*, who reckoned
the earth in the number of the ſtars, and aſcribed
to it a motion about the centre of the world, ſo as
to make the alternative of days and nights ; which
they would not have done, if they had not ſup-
poſed the earth to be nearly of the ſame figure and
bigneſs with the ſtars. This dimenſion related by
*Ariſtotle* might therefore be aſcribed to the *Pytha-
goreans*, who were the authors of this *hypotheſis*.
It is almoſt twice as big as it was afterwards found
by other mathematicians. But it did not appear
large enough at a time when there were yet
ſome philoſophers, who, after *Xenophanes*, queſ-
ſtioned whether it was not of an immenſe magni-
tude.

*Immenſum ne patent terræ ima, et largior æther.*

The appearances of the ſtars related by *Ariſ-
totle*, ſuggeſt two methods of undertaking the mea-
ſure of the earth, which were practiſed in the fol-
lowing ages: One by the obſervations of the ſtars ſi-
tuated vertically in one place, and at a diſtance from
that ſituation in another ; the other by the obſer-

vation of the ſtars in the horizon of one place, and elevated above the horizon of another.

*Eratoſthenes*, under king *Ptolomy Euergetes*, put the firſt method in practice ; he knew that at the time of the ſummer ſolſtice the ſun paſſed over the vertical point of the city of *Syéne*, ſituated in the confines of *Ethiopia* under the tropic of *Cancer*. There was a well made for this obſervation, which on the day of the ſolſtice at noon was quite illuminated by the ſun, and it was notorious that at 150 *ſtadia* round it, the ſtyles raiſed perpendicularly on a horizontal ſurface made no ſhadow ; having ſuppoſed *Alexandria* and *Syéne* to lie under the ſame meridian, he obſerved at *Alexandria* on the day of the ſolſtice the diſtance of the ſun at the vertical point, by the ſhadow of a ſtyle raiſed perpendicularly from the bottom of a concave hemiſphere ; he found that this diſtance was the 50th part of the circumference of a great circle; whence he concluded, that the diſtance between theſe two cities was the 50th part of the circumference of the earth. Having computed this diſtance to be 5000 *ſtadia*, he had the whole circumference 250,000 *ſtadia*.

Having divided it equally into 360 degrees, he had 694 and almoſt ½ to the degree. But afterwards, inſtead thereof he took the round number 700, not thinking perhaps that it would anſwer to 5 or 6 *ſtadia* in a degree.

And multiplying 700 *ſtadia* by 360 degrees, he had in the circumference 252,000 *ſtadia*, which is the laſt dimenſion of *Eratoſthenes*, which he commonly made uſe of. *Hipparchus* alſo made uſe of it, tho he judged that 2520 *ſtadia* ought to be added to this dimenſion.

*Dionoſidorus* only took for the ſemidiameter of the earth ⅙ part of its circumference drawn from

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laſt dimenſion of *Eratoſthenes*, in the letter which was found in his tomb after his death, to make it believed that he had gone down to the centre of the earth, and that having meaſured the diſtance he had found it 42,000 *ſtadia.*

*Vitruvius* and *Pliny* reduce the meaſure of *Eratoſthenes* of 252,000 *ſtadia*, to 31,500 *Roman* miles, allowing 8 *ſtadia* for a mile.

*Poſſidonius*, in the time of *Pompey* the great, undertook to meaſure the circumference of the earth by the ſecond method, that is, by the horizontal obſervations.

He learned that at *Rhodes* the ſtar *Canopus* did but juſt appear at the horizon, and ſet immediately ; and that at *Alexandria*, which he ſuppoſed to be under the ſame meridian, it roſe above the horizon $\frac{1}{48}$ part of the circumference of the heavens, which anſwers to a like part of the circumference of the earth of 7° $\frac{1}{2}$, and ſuppoſing the diſtance between theſe 2 cities to be 5000 *ſtadia*, he had the whole circumference of the earth 240,000 *ſtadia.* This is the firſt dimenſion of *Poſſidonius*, related by *Cleomedes*, an author of the ſame age, adding that it muſt be diminiſhed, if the interval (in *ſtadia*) is not found to be quite ſo great.

*Strabo*, who wrote his geography under *Auguſtus* and *Tiberius*, aſcribes to *Poſſidonius* the dimenſion of the circumference of the earth of 180,000 *ſtadia*, which are in the proportion of 500 *ſtadia* to the degree, it was not eaſy to diſcover the foundation of this ; but here it is. The ſame author teſtifies in another place, that *Eratoſthenes* had meaſured the diſtance between *Rhodes* and *Alexandria* by inſtruments, and found it to be 3750 *ſtadia*, taking it for $\frac{1}{48}$ part of the circumference of the earth according to *Poſſidonius*, there reſult 180,000 *ſtadia.* It may therefore be called

called the laft dimenfion of *Poffidonius*, in which were ufed his dimenfions in degrees, and that of *Eratofthenes* in *ftadia*. It was received by *Marinus Tyrius* the geographer and others. It is commonly afcribed to *Ptolomy*, becaufe he made ufe of it in his geography. We fhall not here relate what is oppofed to thefe methods, we fhall only obferve, as a thing of fome confequence, that having taken exactly the mean between the laft dimenfions of *Eratofthenes* and of *Poffidonius*, we have in a degree of the circumference of the earth 600 *ftadia*, in a minute 10 *ftadia*, which by the account of *Pliny* and *Vitruvius* make 1 mile ¼ of the ancient *Roman* meafure. Now the modern *Italian* mile is equal to 1 mile ¼ of the ancient miles, the diftance of 25 miles, which the ancients reckoned between *Bolonia* and *Modena* being efteemed at prefent to be 20 modern miles. The modern *Italian* mile is 10 *ftadia*, which make a minute, according to the mean dimenfion between that of *Eratofthenes* and *Poffidonius*. The degree of the circumference of the earth will be therefore by this account 60 modern *Italian* miles, and 75 ancient miles. The circumference 21,600 modern miles, 27,000 ancient miles.

Allowing to the mean league 3 ancient miles, we fhall have 25 leagues in a degree, and in the whole circumference 9000 leagues.

After *Eratofthenes* and *Poffidonius*, feveral have made ufe of the heights of the pole, in the meafuring of the earth. The mathematicians of the Caliph *Almamon* having taken the heights of the pole in the plains of *Singar*, at the extremities of two degrees, found 56 miles in one degree, and 56 miles ⅔ in the other, and judged their meafure to be lefs than that of *Ptolemy* by 10 miles. This is very different from all the other dimenfions, which

make

make it much greater. The geographer of *Nubia*, an author of the 12th century, allows 25 leagues to the degree ; this dimenſion was confirmed by that of *Fernellius*, this author made uſe alſo of the heights of the pole taken from the obſervations of the ſun, to find a place nearly under the meridian of *Paris*, which was a degree diſtant from it, and having meaſured the diſtance of it by the wheel, he found it 56747 toiſes, having abated the turns of it at diſcretion. According to the eſtimation of the people of the country, this diſtance was 25 leagues.

*Snellius*, who ſurpaſſed in exactneſs thoſe who had gone before him, made uſe of the heights of the pole obſerved at *Alkmaer* and *Bergen op zoom*, differing between themſelves 1° 11′ ½, and having meaſured the diſtances by the triangles, he found in a degree 56,946 toiſes of 6 *Rhinland* feet each, and by the difference between *Aclmaer* and *Leyden* at the diſtance of ½ a degree, he found 57,020 toiſes in a degree ; He took for a mean between the two 57,000. M. *Picard* conſidering the difference between the *Rhinland* and *Paris* feet, reduces it to 55,021 toiſes.

Above 42 years ago I made ſeveral attempts at *Bolonia* and at *Ferrara*, to meaſure the earth, both by the vertical and horizontal obſervations, which are related by F. *Riccioli* in his geography. It was only to transfer it to the meridian line, which I drew in the great church of St. *Petronius*, to keep account of it, if neceſſary, in the obſervations of the ſun. At the diſtance of 34 toiſes and 2 feet of *Paris*, where the middle of the image of the ſun at the winter ſolſtice is, I marked the 600,000 part of the circumference of the earth, which is in the proportion of 57,222 toiſes to a degree.

F.

F. *Riccoli* and *Grimaldi* took alfo feveral ways and with a great deal of care, the meafure of the earth, as it is related at length in the *Geographia re-formata.*M. *Picard* has reduced it to 64,363 *Paris* toifes to the degree.

But nothing of this kind has ever been done with more care and exactnefs, than what was executed by M. *Picard* in the name of the royal academy of fciences, immediately after its inftitution, in the three firft years.

He meafured exactly by triangles the diftances between the parallels of *Malvoifine, Sourdon*, and *Amiens*, which are but at a little diftance from the meridian of *Paris*, and he obferved with an in-ftrument of 10 feet, the diftances of the fame fixed ftar from the zenith of thefe 3 places. By the diftance between *Malvoifine* and *Sourdon*, he determined the magnitude of a degree to be 57,064 toifes, and by that between *Malvoifine* and *Amiens* of 1° 22′ to be 57,052 toifes. He took for a mean between thefe two 57,060 toifes. The bafe, which he actually meafured is 11 times greater than that of *Snellius*. He ufed in the fame diftance but a fmall number of triangles in proportion to the great number, which had been ufed by *Snellius*. He made ufe of inftruments more finely divided, and furnifhed with tele-fcopes, which augment and diminifh the diftant objects better, and ferve to determine the diftances with a greater exactnefs. With feveral helps he made ufe of the fineft feafons of 3 years. I affifted at feveral of thefe geographical and aftro-nomical obfervations, which we concerted toge-ther.

The meafure of the magnitude of a degree of the circumference of the earth, eftablifhed by the
academy,

academy, might already ferve to correct the differences of longitudes hitherto found by too fhort a meafure. But it was thought neceffary to undertake again under the king's protection, to determine the longitudes of particular places by aftronomical obfervations made at the fame time at the royal obfervatory, and elfewhere. The eclipfes of the moon, which may ferve for this purpofe, being too rare to be able to make any advantage of them in a fhort time, it was propofed to make ufe of the obfervations of the eclipfes of the fatellites of *Jupiter*, of which we had already given tables and *ephimerides* immediately after the inftitution of the academy. For one eclipfe of the moon there happen above a hundred of one fingle fatellite. It was found by experience, that the time of their immerfions into the fhadow of *Jupiter*, and of their emerfions, may be determined with more exactnefs and evidence, than in the eclipfes of the moon. The royal academy immediately attempted this with fome fatisfaction. They at firft had a correfpondence of thefe obfervations with the academy of *Tufcany*, which was then flourifhing, and with the royal fociety of *England*. There was not an aftronomer in *Europe*, who was provided with neceffary inftruments, that did not bear a part in thefe obfervations, and communicate them to the academy. The king fent to have obfervations made not only on the coafts of *France*, but in the moft confiderable places of the 4 quarters of the world. Some religious orders, famous for the profeffion of learning both divine and human, thought of turning thefe obfervations to the fervice of religion in the kingdoms of the *Eaft*, where aftronomy is in reputation. Their obfervations, as well as thofe of moft of the aftronomers of *Europe*,

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compared with thofe which were made at the fame time at the royal obfervatory of *Paris*, have ferved to determine in a fhort time the differences of longitude between this and moft other confiderable cities.

2d. There was not any thing of more importance than to have a meridian well drawn, as that of *Paris* might be drawn thro' all *France*; to take it for bounds of the eaftern and weftern lontudes of all the other places of the earth, as it is the moft illuftrious of the univerfe. The geographers differ among themfelves in the bounds of the meridian of *Paris* in the *Mediterranean*, by the whole extent of the country between the eaftern outlet of the *Rhone*, where *Ptolemy* directs it in his geography, and the city of *Valencia* in *Spain*, thro' which it is drawn by *Hondius* in his map of *Europe*. Was it not neceffary to clear up a point of fo great confequence in geography, and alfo in aftronomy for the reduction of the aftronomical tables from one meridian to another, and for the determination of the eclipfes?

This work was begun by the defcription of the meridian at the obfervatory, both by the obfervations of the fun at the folftices, and by the fixed ftars.

It has been feveral times obferved, and after different manners, and a mean has been taken between the differences. The angles have been taken, which the meridian makes with the diftant places feen at the obfervatory, the diftances of which had already been determined in the firft dimenfion. Perpendiculars have been drawn from thefe places to the meridian, which determine their diftance to eaft and weft, and cut the portion of the meridian intercepted between the parallels

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of thefe places, and of the obfervatory. And in calculating the diftance of thofe places from each other, and the angle of the diftance from the meridian, views have been taken to the places moft advanced to the fouth, where the fame obfervations have been made, always forming triangles of the preceding diftances with the following. Thefe triangles have been continued, without ever interrupting the *feries*, to the fouthern extremity of the *kingdom*. When there did not prefent any remarkable objects proper for the continuation of the triangles, there were trees or pyramids elevated, or other marks vifible at a diftance in the places which were thought the moft proper for this purpofe.

In this manner the meridian was drawn from *Paris* to the higheft mountains of the *Pyrenæans*, which feparate *Rouffillon* from *Catalonia*. The principal towns which are neareft the meridian of *Paris*, where the celeftial obfervations have been made, are *Orleans*, *Aubigny*, *Bourges*, *Aubuffon*, *Aurillac*, *Rhodes*, *Alby*, and *Carcaffone*, which is exactly in the meridian of *Paris*: *Perpignan* and *Colioure* were found much nearer the eaft than was fuppofed. We have not neglected the difference between the lines drawn thro' the high mountains, and the arches of the circumference of the earth which anfwer to them.

In thefe higheft mountains, where it was neceffary to go up, the height of the quickfilver in the barometer was obferved, to compare it with the height obferved at the fame time at *Paris*, and to determine the heights by the comparifon with the obfervations made elfewhere upon mountains, the height of which above the furface of the fea had been meafured by levelling. The fame

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heights have been alfo obferved by means of the diftances from one mountain to another, and of their apparent heights taken by inftruments, continuing thefe obfervations to the fea. The higheft of all thefe mountains is the *Canigou*, which is generally covered with fnow : there had been elevated upon it in the proper feafon, by order of the king, a fort of pyramid, which made it known at a diftance. Its true height, taken from the apparent at the fea-fide, and from its diftance, was found to be 1440 toifes, which are at leaft 15 *ftadia*. *Cleomedes* fays, there is not any mountain, of which the perpendicular height exceeds 15 *ftadia*; this mountain therefore, in the judgment of that author, paffes for one of the higheft.

We fhall not relate here the obfervations which we have made of the great irregularities of the direction of the needle in the mountains of *Limoufin*, *Auvergne*, and *Rouergue*, abounding in mines of iron. We muft not be furprifed if the maps of thefe provinces are ill laid down. They cannot be otherwife, if they are drawn by the compafs, as is commonly done.

We have made fome obfervations of the heights of the pole about our meridian, about all the principal towns, where we made any ftay in a favourable feafon, and particularly in the places determined in our triangles, to compare the differences of the latitudes found in the heavens with thefe dimenfions taken upon the earth. Our dimenfions upon the earth have been confirmed by a bafe of 7200 toifes actually meafured, and by the continuation of the triangles quite to the fea-fide where we took it.

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For fear of parting too far from the meridian in so long a way, we examined it by new obfervations in the places where we were able to do it conveniently, and we have not found any fenfible difference in it greater than what is ufually found, drawing it feveral times by different obfervations in the fame place.

We have had a correfpondence of obfervations of the eclipfes of the fatellites of *Jupiter* made at *Paris*, *Lyons*, *Marfeilles*, at the fame time that we made them about the meridian line, and we have found that the difference of the meridians determined by thefe obfervations agreed with thofe which we had found by the triangles, which have ferved to draw the meridian nearly in the terms as thofe which are made in the fame place by different obfervers agree together. We have made the fame ufe of the eclipfe of the moon of *February* 22, 1701.

And as the difference of the meridians between *Sette* and *Montpellier* had been determined by the obfervations of the fatellites of *Jupiter*, which were made by M. *Picard* in thofe places, compared with thofe which we have made at the fame time at *Paris*, we have been willing to find the difference of thefe meridians by the continuation of the triangles to thofe places, and calculating their diftances from our meridian, and reducing it into minutes and feconds, we have found it alfo to agree with thofe which we had determined by the fatellites of *Jupiter*.

To have the degrees of the circumference of the earth between the parallel of the obfervatory and the bound of our meafure in the *Pyreneans*, we made ufe at *Colioure* of an inftrument of 10 feet *radius*, to obferve feveral fixt ftars near the *Zenith*, and to compare them with thofe which remained

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for us to obferve at *Paris*, by the fame inftrument the next year, in the fame feafon. In the mean time we made ufe of thofe which had been taken at *Paris* by the common inftruments; and we found it to be 6° 18ʹ.

In the hypothefis of the equality of the degrees of the meridian drawn upon the furface of the earth, we fhould have for each minute 954 toifes, 5 feet, 2 inches 5 lines, and for each degree 57,292 toifes. But there are fome very famous mathematicians who queftion this hypothefis, as well as that of the direction of the perpendiculars to the fame point. Since the globe of *Jupiter* has appeared to us a little oval, it has been queftioned whether the earth was not alfo a little oval, and after the obfervations made by the royal academy, at *Cayenne*, *Cape-Verd*, and the *American* iflands, have informed us that the pendulums of the fame length make flower vibrations near the equinoctial than at a greater diftance ; M. *Huygens* and Sir *Ifaac Newton* have endeavoured to explain this *phæno-menon* by a hypothefis, which makes the equinoctial greater than the meridians, fo that its figure would be flatted at the poles ; whereas M. *Eifenfchmid*, by comparing the different dimenfions of the degrees made in different places, to make them agree together, has fuppofed the earth to be oval, longer from one pole to the other than according to the diameter of the equinoctial, making the meridians of an elliptical figure, of which the perpendiculars are directed to different points of the *axis*. He waited however for the fuccefs of our dimenfions, to be able to judge of it with more certainty.

The magnitude of the minute, which we have juft found in the *hypothefis* of the equality, in the interval between *Paris* and the parallel of

*St. Elme*, exceeds that which M. *Picard* found in the fame *hypothefis* in the interval between *Malvoifine* and *Sordon*, which was 951 toifes $\frac{1}{15}$, and ftill more that which is drawn from the obfervations made at *Sourdon*, and *Amiens*, which make the minute 947 toifes. The excefs of ours above the firft, is almoft 4 toifes, 5 feet; above the fecond, 7 toifes, and almoft 5 feet: thus the further we recede from the equinoctial, the more the dimenfions upon the earth diminifh, which feems favourable to the modern *hypothefes*, which we have juft related, it is difficult to find any one that makes thefe differences agree fufficiently together.

It appears however by the comparifons which we have made, that in the fpace between the parallels of *Amiens* and *Colioure*, which is 7° and above $\frac{1}{3}$, the meafure of the degrees going toward the equinoctial increafes, fo that the following degree exceeds the preceding by its 800th part, which in this interval varies from 70 toifes and 2 feet, to 72 toifes, and 1 foot. The degree taken from the obfervatory towards the north gives us 57,055 toifes, and the degree taken from the obfervatory towards the fouth gives 57,126 toifes $\frac{1}{2}$. By this progreffion the magnitude of the other degrees will be found till that which it continues uniformly. The minute from the obfervatory towards the north is 951 toifes, 5 feet $\frac{1}{2}$. It is equal to the diftance between the obfervatory and the church of *St. Severin*, and may pafs for the fmalleft diameter of *Paris*. Toward the fouth, the minute from the obfervatory is 951 toifes, 5 feet, 7 inches $\frac{1}{2}$, and the augmentation of 1 inch, $\frac{11}{25}$, in each minute amounts to 72 toifes from one degree to another.

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We have been furprifed to fee that this augmentation of $\frac{1}{800}$th part from one degree to another, at this diftance from the poles, agrees with the augmentation of the degrees of the true diftance of the moon in its *apogeum* from the fame degrees of diftance between 40 and 48 degrees, from the pole on one fide, and the *apogeum* of the moon on the other. For in this place the degrees of the true diftance of the moon in its *apogeum* increafe alfo from one to another $\frac{1}{800}$th part. Thus the perpendicular lines, which terminate the degrees meafured in the heavens, would be analogous to the lines of the mean longitudes of the moon, and the arches of the circumference of the earth between thefe perpendiculars would be of the fame magnitude as the arches of the fame circumference contained between the lines correfponding with thofe of the mean motion of the moon.

If this equality, which is found in thefe 7 degrees of the circumference of the earth, of which we have the dimenfions, is found in the whole circumference; it is what we dare not advance, tho' the moon has fome fhare in the *equilibrium* of the fea, to which it brings fome variation evident in the flux and reflux regulated by its motions. It would be a fact to be verified by dimenfions of a greater extent, if other princes would contribute as much as the king to the perfection of the fciences.

*F  I  N  I  S.*